

Welcome Austric members!

I'm posting relevant parts of a message I received from Dr. Francesco Brighenti.

Some comments i would like to add to his observations:

\* Megalithic culture is also found among the Igorots of the Philippines who use paved stone platforms known as ato along with standing stones and other megaliths like stone circles, backrests, seats, etc. The water buffalo sacrifice is also found widely among these peoples but I don't know if this pattern fits into the same pattern as described by Brighenti.

\* In Sumba and other areas of E. Indonesia, the horse sacrifice is used at funerals with the same idea of the horse's soul accompanying the deceased to the afterlife.

Regards,  
Paul Kekai Manansala

---begin forwarded message

Wheeler used Fuerer-Haimendorf's theory while discussing the origin of the megalithic traits occurring in some tribal cultures of eastern India. Although rejecting any connection between the first millennium BCE megalithic archaeological complex of South India, which he ascribes to the ancient Dravidians, and the megalithic cultures of Indonesia, whose archaeological remains apparently all date to later epochs, Wheeler yet accepts Fuerer-Haimendorf's hypothesis about a cultural bridge which would link the megalithic rituals diffused among some modern Austroasiatic-, Dravidian- and Tibeto-Burman-speaking tribes of central-eastern and northeastern India, settled along the Eastern Ghats, on the Chota Nagpur Plateau and in the Assamese area, to the structurally similar megalithic rituals diffused among some Austronesian-speaking tribes of Indonesia. One of the possible ethnic vehicles of such a process of cultural interaction would be represented, in the two scholars' hypothesis, by the Austroasiatic-speaking tribes of India and mainland Southeast Asia.

I made some ethno-archaeological research in this line, being focused on the sacrifices of bovines - mainly buffaloes, but also zebu cows and bulls and, along the Indian-Burmese border, mithuns ? which are

still today customarily performed, on the occasion of either 'green' and 'dry' funerals, by a large number of Indian, South Chinese, Indo-Chinese and Indonesian tribal communities. My purpose was to verify the possibility of the existence of a common ritual prototype for such funerary sacrifices. I found that many a religio-cultural element is shared in common by most of the tribes who use to sacrifice bovines in order to satisfy the dead relatives and the ancestral spirits. In my opinion, the most important among these religio-cultural elements are the following:

1)

The role of supernatural carrier of the dead's soul attributed to the sacrificial bovine, and particularly to the water-buffalo;

2)

The very common association of megalithic rites (erection of menhirs, dolmens, memorial poles, etc.) with the tribal funerary ceremonies involving the sacrifice of bovines;

3)

The ritual installation of the horns or the skulls of the sacrificial bovines on such megalithic monuments, whether they belong to the memorial or to the burial class;

4)

The frequent use of forked (Y-shaped) posts, resembling bovine horns in their shape, in connection with such mortuary ceremonies;

5)

The presence of the stylised motif of the bovine horns in the architectural traditions of many among the tribes who immolate bovines to the dead/ancestors, with this feature being widespread, in particular, in Assam/Burma and in Indonesia (please note that, especially in Indonesia, tribal longhouses are decorated with real or wooden buffalo horns and are commonly identified with the clan's ancestors and, in some cases, with the Sacred Buffalo symbolising the same);

6)

The socio-economic functions 'e.g., in the redistribution of cattle wealth and the redefinition of a family's kinship links with their own clan on the basis of the major or minor lot of cattle wealth offered to common ancestors' which is attributed to the funerary sacrifices of bovines by some of the tribal groups at issue, with the best examples being represented by the Naga of the Assamese area (whose well-known Feasts of Merit are associated, in certain instances, with

ancestor-cult), by the Gadaba and Lanjia Saora of Orissa and, as far as Indonesia is concerned, by the Toraja of Sulawesi and by the tribes of Sumba.

I do not envisage any relevant influence of Vedic religious beliefs or practices on this widespread class of tribal mortuary rituals. True, the Brahmana texts describe the sacrifice of the Anustarani cow besides the funeral pyre, and the donation of the Vaitarani cow to the Brahmins, as parts of two different, and yet interrelated, Vedic funeral ceremonies, but nevertheless these texts are absolutely silent about some form of buffalo-sacrifice being ever performed on the same ritual occasion, as well as about the water-buffalo's role - found among many tribal groups of India and Southeast Asia - as the carrier of the soul of the dead to the afterlife. This role, on the contrary, is reserved in the Brahmana literature ? and also in the actual ritual of disposal of the dead followed by some Hindu castes till a few decades ago - to the Vaitarani only, that is, a FEMALE ANIMAL OFFERED TO THE BRAHMANAS, AND NOT OFFERED AS A SACRIFICE, which, in my opinion, is something very different from the MALE ANIMAL (water-buffalo, mithun or zebu) that is actually OFFERED AS A SACRIFICE in the course of the funeral rites observed by the tribal groups of India and Southeast Asia I have researched upon.

On the other hand, the Shakta form of buffalo-sacrifice ? namely, the one and only known form of a buffalo-sacrifice which can historically have influenced the sacrificial traditions evolved out in different tribal contexts from India to Southeast Asia ? is not related to funeral ceremonies and cannot, therefore, be considered to be the Tantric archetype for the sacrifices of bovines performed by those tribal groups in connection with their own mortuary/megalithic rituals.

Hence a series of questions. Which, if any, was the ultimate ethno-cultural source for the sacrifices of buffaloes/mithuns/zebus associated, both in India and in Southeast Asia, with tribal ceremonies of first and second funeral and, more in general, with tribal ancestor-worship? Out of which religious ideas did such a sacrificial complex ? if I am allowed to describe it as a unity ? evolve? Was there any core geographical area where this sacrificial tradition developed in an undeterminable epoch? Is this area - as one may conjecture given the wide diffusion of tribal funerary sacrifices of bovines from central-eastern India to Vietnam and from southern China to Sulawesi ? to be located somewhere between southern China and northern Indo-China, which are considered by some scholars to have represented the most important areas from which the dispersion of Austroasiatic, Austronesian, Miao-Yao and Tai-Kadai language families started?

To this I would like to add another crucial theme of debate: has the Hindu god of death Yama's association with the water-buffalo, an animal acting as his vahana, anything to do with the funerary observances peculiar to the tribal peoples of India and Southeast Asia who sacrifice buffaloes, or other bovines, during their death rituals? In other words, did Yama's association with the water-buffalo, through a process of acculturation vehicled by the eastward expansion of Indian civilization, contribute to generate this class of tribal sacrifices of bovines connected with death rituals, or was it the reverse? Or, again, may some kind of a religio-symbolic association of the water-buffalo with the concept about death - one which, in the pre-historic epoch, was possibly recognized throughout tropical Asia ? have influenced both the tribal mortuary observances in discussion and the specifically Vedic relationship uniting the god of death Yama to the water-buffalo?

The debate is open!

| 2|2002-05-30 07:55:36|frabrig|Re: Water buffalo sacrifice in Southeast Asia and India|

--- In austrie@y..., "pinatubo.geo" wrote:

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> I'm posting relevant parts of a message I received from Dr.

> Francesco Brighenti.

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> Some comments i would like to add to his observations:

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> is used at funerals with the same idea of the horse's soul

> accompanying the deceased to the afterlife.

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> Regards,

> Paul Kekai Manansala

Unfortunately my message contained a lot of typos, caused by its conversion from Winword format into html.

The correct form of the message goes as follows:

Wheeler used Fuerer-Haimendorf's theory while discussing the origin of the megalithic traits occurring in some tribal cultures of eastern India. Although rejecting any connection between the first millennium BCE megalithic archaeological complex of South India, which he ascribes to the ancient Dravidians, and the megalithic cultures of Indonesia, whose archaeological remains apparently all date to later epochs, Wheeler yet accepts Fuerer-Haimendorf's hypothesis about a cultural bridge which would link the megalithic rituals diffused among some modern Austroasiatic-, Dravidian- and Tibeto-Burman-speaking tribes of central-eastern and northeastern India, settled along the Eastern Ghats, on the Chota Nagpur Plateau and in the Assamese area, to the structurally similar megalithic rituals diffused among some Austronesian-speaking tribes of Indonesia. One of the possible ethnic vehicles of such a process of cultural interaction would be represented, in the two scholars' hypothesis, by the Austroasiatic-speaking tribes of India and mainland Southeast Asia.

I made some ethno-archaeological research in this line, being focused on the sacrifices of bovines -- mainly buffaloes, but also zebu cows and bulls and, along the Indian-Burmese border, mithuns -- which are still today customarily performed, on the occasion of either \*green\* and \*dry\* funerals, by a large number of Indian, South Chinese, Indo-Chinese and Indonesian tribal communities. My purpose was to verify the possibility of the existence of a common ritual prototype for such funerary sacrifices. I found that many a religio-cultural element is shared in common by most of the tribes who use to sacrifice bovines in order to satisfy the dead relatives and the ancestral spirits. In my opinion, the most important among these religio-cultural elements are the following:

- 1) The role of supernatural carrier of the dead's soul attributed to the sacrificial bovine, and particularly to the water-buffalo;
- 2) The very common association of megalithic rites (erection of menhirs, dolmens, memorial poles, etc.) with the tribal funerary ceremonies involving the sacrifice of bovines;

3) The ritual installation of the horns or the skulls of the sacrificial bovines on such megalithic monuments, whether they belong to the memorial or to the burial class;

4) The frequent use of forked (Y-shaped) posts, resembling bovine horns in their shape, in connection with such mortuary ceremonies;

5) The presence of the stylised motif of the bovine horns in the architectural traditions of many among the tribes who immolate bovines to the dead or the ancestors, with this feature being widespread, in particular, in Assam/Burma and in Indonesia (please note that, especially in Indonesia, tribal longhouses are decorated with real or wooden buffalo horns or with buffalo-heads made of straw, and they are commonly identified with the clan's ancestors and, in some cases, with the Sacred Buffalo symbolising the same);

6) The socio-economic functions -- e.g., in the redistribution of cattle wealth and the redefinition of a family's kinship links with their own clan on the basis of the major or minor lot of cattle wealth offered to common ancestors -- which is attributed to the funerary sacrifices of bovines by some of the tribal groups at issue, with the best examples being represented by the Nagas of the Assamese area (whose well-known Feasts of Merit are associated, in certain instances, with ancestor-cult), by the Gadabas and Lanjia Saoras of Orissa and, as far as Indonesia is concerned, by the Torajas of Sulawesi and by the tribes of Sumba.

I do not envisage any relevant influence of Vedic religious beliefs or practices on this widespread class of tribal mortuary rituals. True, the \*Brahmana\* texts describe the sacrifice of the \*anustarani\* cow besides the funeral pyre, and the donation of the \*vaitarani\* cow to the Brahmins, as parts of two different, and yet interrelated, Vedic funeral ceremonies, but, nevertheless, these texts are absolutely silent about some form of buffalo-sacrifice having ever been performed in Vedic times on the same ritual occasion, as well as about the water-buffalo's role -- found among many tribal groups of India and Southeast Asia -- as the carrier of the soul of the dead to the afterlife. This role, on the contrary, is reserved in the \*Brahmana\* literature -- as also in the actual ritual of disposal of the dead followed by some Hindu castes till a few decades ago -- to the \*vaitarani\* only, that is, a FEMALE ANIMAL OFFERED TO THE BRAHMINS, AND NOT OFFERED AS A SACRIFICE, which, in my opinion, is something very different from the MALE ANIMAL (water-buffalo, mithun or zebu) that is ACTUALLY OFFERED AS A SACRIFICE in the course of the funeral rites observed by the tribal groups of India and Southeast

Asia I have researched upon.

On the other hand, the Shakta form of buffalo-sacrifice -- namely, the one and only known form of a buffalo-sacrifice which can historically have influenced the sacrificial traditions having evolved out in different tribal contexts from India to Southeast Asia -- is \*not\* related to funeral ceremonies and cannot, therefore, be considered to be a Tantric archetype for the sacrifices of bovines performed by those tribal groups in connection with their own mortuary/megalithic rituals.

Hence a series of questions. Which, if any, was the ultimate ethno-cultural source for the sacrifices of buffaloes/mithuns/zebus associated, both in India and in Southeast Asia, with tribal ceremonies of first and second funeral and, more in general, with tribal ancestor-worship? Out of which religious ideas did such a sacrificial complex -- if I am allowed to describe it as an unity -- evolve? Was there any core geographical area where this sacrificial tradition developed in an undeterminable epoch? Is this area, as one may conjecture given the wide diffusion of tribal funerary sacrifices of bovines from central-eastern India to Vietnam and from southern China to Sulawesi, to be located somewhere between southern China and northern Indo-China, which are considered by some scholars to have represented the most important areas from which the dispersion of Austroasiatic, Austronesian, Miao-Yao and Tai-Kadai language families started?

To this I would like to add another crucial theme of debate: has the Hindu god of death Yama's association with the water-buffalo, an animal acting as his \*vahana\*, anything to do with the funerary observances peculiar to the tribal peoples of India and Southeast Asia who sacrifice buffaloes, or other bovines, during their death rituals? In other words, did Yama's association with the water-buffalo, through a process of acculturation vehicled by the eastward expansion of Indian civilization, contribute to generate this class of tribal sacrifices of bovines connected with death rituals, or was it the reverse?

Or, again, may some kind of a religio-symbolic association of the water-buffalo with the concept about death (one which, in the pre-historic epoch, was possibly recognized throughout tropical Asia) have influenced both the tribal mortuary observances in discussion and the specifically Vedic relationship uniting the god of death Yama to the water-buffalo?

The debate is open!

Cheers,  
Francesco Brighenti

| 3|2002-05-30 12:19:55|frabrig|Re: Water buffalo sacrifice in Southeast Asia and India|

--- In austric@y..., "frabrig" wrote:

> --- In austric@y..., "pinatubo.geo" wrote:  
> >  
> > \* Megalithic culture is also found among the Igorots of the  
> > Philippines  
> > who use paved stone platforms known as ato along with standing  
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> > is used at funerals with the same idea of the horse's soul  
> > accompanying the deceased to the afterlife.  
> >  
> > Regards,  
> > Paul Kekai Manansala

Your comments are pertinent in this context, although I would like to focus the discussion specifically on the archaic cultural links uniting some ethnically heterogeneous tribes of India, Indo-China and Indonesia who still today immolate buffaloes in honour of the dead and/or the ancestral spirits in the ambit of their death and/or ancestor-worship rituals.

Regarding the Igorots, I may benignly mention here that last year I had a long discussion online with some ethnic Igorots based in the U.S.A. about their own tradition of carabao/karabau-sacrifice. The pattern in this case appears to differ from the one being typified by the funerary sacrifices performed by some tribal groups of India, Indo-China and Indonesia. For instance, my Igorot informants denied carabaos to be sacrificed in Luzon on the occasion of tribal funerals. They even denied carabaos to be actually \*sacrificed\* at all, for, they said, these large animals are just killed in order to provide abundant meat on the occasion of certain communal feasts. An American professor wrote me that carabao-sacrifice cannot be so much



an age-old tradition in the Luzon Cordillera as much as buffalo-sacrifice is in Indonesia, because carabaos, according to her researches, was imported from the plains into the mountain territories inhabited by the Igorot tribes only after the Spanish conquered the Philippines. I can provide some references about this topic to anyone interested.

Regarding the horse-sacrifices performed in Indonesia during funeral ceremonies, don't you think this tribal ritual tradition is of more recent origin than that pivoted upon buffalo-sacrifice?

Regards,  
Francesco Brighenti

| 4|2002-05-30 12:44:47|a.manansala@attbi.com|Re: Water buffalo sacrifice in Southeast Asia and India|

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> > --- In austri@y..., "pinatubo.geo" wrote:  
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> conquered the Philippines. I can provide some references about this  
> topic to anyone interested.  
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It may be that the carabao sacrifice, if indeed that is what it is, is not related to that in Indonesia.

As for the carabao being imported only after the arrival of the Spanish is that based on Fox's theories. Personally, I don't think much of his dating of the rice terraces.

Btw, some more recent radiometric testing from this area tends to support Beyer's dating more than Fox's.

How do the Igorot megaliths compare with those in Indonesia? Those used in the Philippines seem mostly used for communal gatherings. I don't know if they have any special connection with funerary rites.

> Regarding the horse-sacrifices performed in Indonesia during funeral  
> ceremonies, don't you think this tribal ritual tradition is of more  
> recent origin than that pivoted upon buffalo-sacrifice?  
>

It could be. Horses are depicted on early Dong-son-like bronzes from Sunda. There is a strong argument for an ancient presence of horses in SE Asia.

Regards,  
Paul Kekai Manansala  
<http://home.attbi.com/~a.manansala/vedicindia.html>

| 5|2002-05-31 00:54:58|LV Hayes|Related Web Sites|

The list of related web sites should include my own at  
<http://home.att.net/~lvhayes/home.htm>

This site includes a large section on Austric linguistics. See the Table of Contents on this page for available links.

By Austric, I mean the language superfamily envisioned by Wilhelm Schmidt in 1906 as comprising the Austroasiatic and Austronesian language families. Sufficient linguistic evidence exists to confirm the existence of this Austric; other concepts of what language families Austric should include, such as those of Pejros and Ruhlen, remain to be proven in my opinion.

LV Hayes  
<[lvhayes@worldnet.att.net](mailto:lvhayes@worldnet.att.net)>

--- In austric@y..., a.manansala@a... wrote:

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- > it is, is not related to that in Indonesia.
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- > I don't think much of his dating of the rice terraces.
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- > Btw, some more recent radiometric testing from this area
- > tends to support Beyer's dating more than Fox's.

Please write more about Fox's and Beyer's datings of the rice terraces in the Luzon Cordilleras and their relation to the advent of carabao breeding in that area. I am very interested in knowing more about this subject.

In a personal e-mail message posted to the present writer on 16 Nov 2001, Prof. Patricia Afable, Research Associate, Asian Cultural History Program, NMNH, Smithsonian Institution, Washington DC wrote:

"I believe that the use of water buffalo... in rituals of any sort in the northern Philippines is recent, and is primarily associated with the need to feed large numbers of visitors... Carabaos were generally brought from the lowlands... The Ibaloy people in the southern Cordillera, along with the Kalanguya- and I'uwak-speaking people, also have large funerary feasts, and carabaos as well as cows are slaughtered at death feasts of rich people. The Ibaloy started raising cattle in the Spanish period... Carabaos are also 'offered' (i.e. through invocation) to the ancestors, but the pigs must be offered first. There are no rituals that involve only carabaos alone (or cows for the matter). That is what makes me believe that it is the necessity of feeding large crowds that has brought in the carabao and the cow. A hundred years ago, large animals like carabaos or cattle were very scarce in the Luzon highlands, except in the Benguet (Ibaloy and Kankanaey) area, where cattle ranching was quite common. These large livestock are called 'animal' (from the Spanish word, with the accent on the last syllable) in these languages, for example, which is a sign that they were considered foreign and from the outside. Therefore, although the peoples of the Benguet area had cattle for much longer than everyone else, they have not really incorporated it into their ritual. In fact, among some families, when

they kill a cow or carabao for a large feast, they kill a small pig first to use for invoking the spirits they want to reach. This is a real proof (for me anyway) that they can not reach the ancestors except through the traditional ritual animals... Note that in upland areas, it is not always possible to bring carabaos into the rice fields because the terrace stone walls are too high" (referenced to P. Afable's article "Mortuary Ritual among the Ibaloy", *\*Asian Folklore Studies\** Vol. 34-2, 1975).

However, other Igorot informants of mine maintain that carabaos are killed in elaborate funerary rituals in northern Luzon. Cases are recorded in Scott's *\*Complete Works\** of some Kalinga dying persons seeing a carabao-like apparition -- a hint at the carabao's role as a superbatural soul-carrier, a role which is similarly attributed to the water-buffalo by a number of Indonesian, Indo-Chinese and Indian tribal peoples? Another informant of mine reported he witnessed a carabao-sacrifice during a burial ceremony in the Benguet area of Luzon. In answer to his inquiry, relatives of the deceased explained that the carabao will pull the coffin to the destination of the dead yonder.

Thus, once again, my query is: what has this kind of a carabao-butcher activity in common with the analogous sacrifices of buffaloes performed by some Indonesian tribes on the occasion of funeral ceremonies? Is this a recent tradition or an archaic one? Can these funerary sacrifices of Luzon be compared to those occurring among the tribes of Sulawesi and Sumba? Is there any common religious strain below? If so, can such a supposed common religious strain be defined as peculiar to the proto-Austronesians?

- > How do the Igorot megaliths compare with those in Indonesia?
- > Those used in the Philippines seem mostly used for communal
- > gatherings. I don't know if they have any special connection
- > with funerary rites.

I frankly know little or nothing about the Igorot megalithic traditions. Please write more about this topic.

- >Horses are depicted on early Dong-son-like bronzes from Sunda. There
- >is a strong argument for an ancient presence of horses in SE Asia.

Thank you for this precious piece of information. I am aware that the ceremonies of secondary burial of the Sumbanese noblemen, on the occasion of which megalithic tombs are erected, culminate in the mass slaughter of water-buffaloes, horses, pigs and dogs, followed by grandiose funerary banquets during which the meat of such animals is eaten by large crowds of people.

The role of soul-carrier (to the afterworld) is attributed by the Sumbanese to the water-buffalo and the horse alike. Therefore, the tombs of the noblemen in Sumba are carved with images of both buffaloes (normally only the head of the horns of the animal) and horsemen.

The horse has also a fundamental role in the classic shamanistic religion of the Batak tribes of northern Sumatra. They used to breed sacred horses for sacrificial purposes only. These animals appear to have been customarily slaughtered in the past in the ambit of ancestor-worship rituals performed by the descendants of common ancestors called \*begu\*. For discussion, check the following web page:

[http://www.holocaustrevealed.org/english/s/b7\\_9html](http://www.holocaustrevealed.org/english/s/b7_9html)

To conclude this long post, I invite you not to lose sight of the original thread, that is, the supposed (by me anyway) relationship uniting the funerary sacrifices of buffaloes being still today performed by a chain of tribal peoples settled in the Indian sub-continent, southern China, Indo-China and Indonesia (adding perhaps the Philippines too). No specific discussion about my argument has started so far. Any comment on my original post?

Regards,  
Francesco Brighenti

| 7/2002-05-31 09:07:22|frabrig|Re: Water buffalo sacrifice in Southeast Asia and India|

--- In austriac@y..., "frabrig" wrote:

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Oops! Sorry, the correct URL is:

[http://www.ccg.org/english/s/B7\\_9.html](http://www.ccg.org/english/s/B7_9.html)

Regards,  
Francesco Brighenti

| 8|2002-05-31 09:44:54|pinatubo.geo|Re: Related Web Sites|

I've added your site to the list. If anyone knows of any other sites, there's still space for at least a few more.

Regards,  
Paul Kekai Manansala

--- In austri@y..., LV Hayes wrote:

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> in 1906 as comprising the Austroasiatic and Austronesian language families.

> Sufficient linguistic evidence exists to confirm the existence of this

> Austric; other concepts of what language families Austric should include,

> such as those of Pejros and Ruhlen, remain to be proven in my opinion.

>

> LV Hayes

>

| 9|2002-05-31 11:27:20|a.manansala@attbi.com|Re: Water buffalo sacrifice in Southeast Asia and India|

> --- In austri@y..., a.manansala@a... wrote:

>

- > > Btw, some more recent radiometric testing from this area
- > > tends to support Beyer's dating more than Fox's.
- >
- > Please write more about Fox's and Beyer's datings of the rice
- > terraces in the Luzon Cordilleras and their relation to the advent of
- > carabao breeding in that area. I am very interested in knowing more
- > about this subject.
- >

Fox theorized the terraces were built only after the coming of the Spanish when the Igorots were forced into the mountains. I think that's hard to establish on linguistic grounds. It seems to me that the terraces are connected with very old migrations and region-wide terrace-building know-how. It may even be that the original terrace builders were only root farmers (taro, yams, etc.) and did not yet know about rice agriculture.

Beyer dated the terraces to at least 1,000 BCE based on diggings he made in the area.

There is one Philippine researcher who dates the terraces to pre-fourth millennium BCE based on geological dating. Unfortunately, I can't remember his name.

- > Thus, once again, my query is: what has this kind of a carabao-
- > butchering activity in common with the analogous sacrifices of
- > buffaloes performed by some Indonesian tribes on the occasion of
- > funeral ceremonies? Is this a recent tradition or an archaic one? Can
- > these funerary sacrifices of Luzon be compared to those occurring
- > among the tribes of Sulawesi and Sumba? Is there any common religious
- > strain below? If so, can such a supposed common religious strain be
- > defined as peculiar to the proto-Austronesians?
- >

Most Igorots now have been Christianized or partially-Christianized. Maybe the butchering of buffaloes and other animals has lost the original religious significance in many of the Igorot areas.

For example, I've seen Christian wedding ceremonies in the

Ifugao region that were mostly indigenous in ritual but were the religious significance had been shifted toward Christianity.

Maybe some linguistic investigation might indicate whether the buffalo sacrifice extends back to the proto-Austronesian period.

- > > How do the Igorot megaliths compare with those in Indonesia?
- > > Those used in the Philippines seem mostly used for communal
- > > gatherings. I don't know if they have any special connection
- > > with funerary rites.
- >
- > I frankly know little or nothing about the Igorot megalithic
- > traditions. Please write more about this topic.
- >

The latest work on these megaliths that I'm aware of is H.H.E. Loofs "Some remarks on the 'Philippine megaliths' in Asian Studies". Unfortunately, I don't have the exact citation right now, but I'll try to get that for you.

Loofs theorizes the megaliths may be related to Cham or Mon influence. He notes though that there has been no systematic study of megaliths in the Philippines as compared to Indonesian and Indochina.

The Philippine megaliths consist of stone platforms known as *ato*, which are associated with upright stones that may be up to 1.5 meters high. There are also huge flat circular stones known as *dap-ay*. There are also stone backrests known as *handangan* that bear close resemblance to similar backrests among the Yami of Taiwan.

A megalithic site has also been found in the island of Iloilo in the Visayas.

Here is an abstract of a relevant article:

#### QUOTE

Geological Study on the Megalithic Structures in  
Agsalanan, Dingle, Iloilo  
by Luis Omana



A geological study on the reported "Megalithic Structure" in Aagsalanan, Dingle, Iloilo was conducted in order to determine the nature, occurrence and to assemble a comprehensive knowledge of the geology of the area which will be used as a basis in determining if said structure is a natural geologic formation or not. Structural, lithological and rock weathering were reviewed during the course of investigation.

Two phases of study were recommended: 1) stratigraphic correlation which yielded positive of these so called "Megalithic structures"; and 2) deep excavation far away from Aagsalanan digging 2500 meters away to find out an extension of the same rocks.

UNQUOTE

National Museum Papers (1992), Vol. 3 No 2

> >Horses are depicted on early Dong-son-like bronzes from Sunda. There  
> >is a strong argument for an ancient presence of horses in SE Asia.  
>

> Thank you for this precious piece of information. I am aware that the  
> ceremonies of secondary burial of the Sumbanese noblemen, on the  
> occasion of which megalithic tombs are erected, culminate in the mass  
> slaughter of water-buffaloes, horses, pigs and dogs, followed by  
> grandiose funerary banquets during which the meat of such animals is  
> eaten by large crowds of people.

>  
> The role of soul-carrier (to the afterworld) is attributed by the  
> Sumbanese to the water-buffalo and the horse alike. Therefore, the  
> tombs of the noblemen in Sumba are carved with images of both  
> buffaloes (normally only the head of the horns of the animal) and  
> horsemen.

>  
> The horse has also a fundamental role in the classic shamanistic  
> religion of the Batak tribes of northern Sumatra. They used to breed  
> sacred horses for sacrificial purposes only. These animals appear to  
> have been customarily slaughtered in the past in the ambit of  
> ancestor-worship rituals performed by the descendants of common  
> ancestors called \*begu\*. For discussion, check the following web page:

>  
> [http://www.holocaustrevealed.org/english/s/b7\\_9html](http://www.holocaustrevealed.org/english/s/b7_9html)  
>  
> To conclude this long post, I invite you not to lose sight of the  
> original thread, that is, the supposed (by me anyway) relationship  
> uniting the funerary sacrifices of buffaloes being still today  
> performed by a chain of tribal peoples settled in the Indian sub-  
> continent, southern China, Indo-China and Indonesia (adding perhaps  
> the Philippines too). No specific discussion about my argument has  
> started so far. Any comment on my original post?  
>

There are some who believe that Proto-Austronesian people erected megaliths since these are found all over SE Asia and Oceania. Since megaliths are also widely found among Austroasiatic people, it may be possible that this practice dates back to the Proto-Austrian period, although the archaeological evidence is lacking.

I wonder what a linguistic investigation would uncover?

Regards,  
Paul Kekai Manansala

| 10|2002-05-31 11:49:15|pinatubo.geo|Re: Water buffalo sacrifice in Southeast Asia and India|

--- In austrian@y..., a.manansala@a... wrote:

>  
> > --- In austrian@y..., a.manansala@a... wrote:  
> >  
>  
  
>  
> I wonder what a linguistic investigation would uncover?  
>

One possibility with regard to Austronesian. There is a vague resemblance between the Igorot word "ato" for stone platforms associated with upright stones, and the Polynesian "ahu"

for stone platforms associated with menhirs. I'll try to followup on this when I have time.

Regards,  
Paul Kekai Manansala

| 11|2002-06-01 02:36:51|chingdude56|south east asian sea levels during last ice age~|

from doug weller's website:

<http://www.ramtops.demon.co.uk/migrate.html>

| 12|2002-06-02 11:10:43|pinatubo.geo|Water buffalo domestication|

The date and place of water buffalo domestication may be relevant to the current discussion.

The dates for the swamp buffalo vary from 5000 to 2500 BCE.

An abstract from the latest research:

Anim Genet 1998 Aug;29(4):253-64

Genetic diversity of Asian water buffalo (*Bubalus bubalis*):  
mitochondrial DNA D-loop and cytochrome b sequence variation.

Lau CH, Drinkwater RD, Yusoff K, Tan SG, Hetzel DJ, Barker JS.

Department of Biology, Universiti Putra Malaysia, Serdang, Malaysia.

Swamp and river buffalo mitochondrial DNA (mtDNA) was sequenced for 303 bp of the cytochrome b gene for 54 animals from 14 populations, and for 158 bp of the D-loop region for 80 animals from 11 populations. Only one cytochrome b haplotype was found in river buffalo. Of the four haplotypes identified in swamp buffalo, one found in all populations is apparently ancestral both to the other swamp haplotypes and to the river haplotype. The phylogenetic relationships among the 33 D-loop haplotypes, with a cluster of 11 found in swamp buffalo only, also support the evolution of domesticated swamp and river buffalo from an ancestral swamp-like animal, most likely represented today by the wild Asian buffalo (*Bubalus arnee*). The time of divergence of the swamp and river types, estimated from the D-loop data, is 28,000 to 87,000 years ago. We hypothesise that the species originated in mainland south-east Asia, and that it spread north to China and west to the Indian subcontinent, where the river type evolved and was domesticated. Following domestication in China, the domesticated swamp buffalo spread through two separate routes, through

Taiwan and the Philippines to the eastern islands of Borneo and Sulawesi, and south through mainland south-east Asia and then to the western islands of Indonesia.

| 13|2002-06-04 07:31:32|sferrarini|Pearic Peoples of Cambodia|

Dear Sirs,

I write from Italy and i've a great interest for austroasiatic peoples. Particularly, in these last times i'm looking for recent news and images related to some small cambodian ethnic groups belonging to the Pearic branch of austroasiatic family. I refer to "Parr" and "Chong" who live near the Cardamom chain, in addition to "Samr" 𑜇𑜡 living close to koulen mountains and the "Saoh" located not far from Elephant mountains. Many anthropologists have classified them in the past as "veddoid" (mainly Parr and Samr 𑜇𑜡) and "negritoid" (mainly Saoh) or a mixture between them. I've found some old documents about these populations, mainly of 40's. I think it should be very interesting to compare these ones with recent ones to verify if they have been absorbed by khmer society or they have kept their cultural identity and their physical features. Therefore i would like to know if you hold any recent (i mean throughout last 10 years) resources about these interesting and little-known peoples. Please let me know. Also, i would ask what's your opinion about the whole austro-asiatic stock of peoples: do you think they took origin by a common place from where they moved towards two different routes (India on the one hand and South-East Asia on the other side)? or rather do you think they originally occupied the whole tropical Asia area and afterwards they were scattered by indo-aryan and mongolic invasions?

Thank you very much for your attention and my best regards,

Stefano Ferrarini

| 14|2002-06-05 20:30:41|pinatubo.geo|Re: Pearic Peoples of Cambodia|

--- In austric@y..., "sferrarini" wrote:

> Dear Sirs,

>

> I write from Italy and i've a great interest for austroasiatic

> peoples. Particularly, in these last times i'm looking for recent

> news and images related to some small cambodian ethnic groups

> belonging to the Pearic branch of austroasiatic family.

Here's one image of the Pearr I was able to find on the web:

<http://angkor.com/pearr.jpg>

I refer

> to "Pearr" and "Chong" who live near the Cardamom chain, in addition  
> to "Samr" living close to Kouleu mountains and the "Saoch" located  
> not far from Elephant mountains. Many anthropologists have classified  
> them in the past as "veddoid" (mainly Pearr and Samr) and  
> and "negritoid" (mainly Saoch) or a mixture between them. I've found  
> some old documents about these populations, mainly of 40's. I think  
> it should be very interesting to compare these ones with recent ones  
> to verify if they have been absorbed by Khmer society or they have  
> kept their cultural identity and their physical features. Therefore I  
> would like to know if you hold any recent (I mean throughout last 10  
> years) resources about these interesting and little-known peoples.  
> Please let me know. Also, I would ask what's your opinion about the  
> whole Austro-Asiatic stock of peoples: do you think they took origin  
> by a common place from where they moved towards two different routes  
> (India on the one hand and South-East Asia on the other side)? or  
> rather do you think they originally occupied the whole tropical Asia  
> area and afterwards they were scattered by Indo-Aryan and Mongolic  
> invasions?  
>

I believe that if the Proto-Austrian period was sometime between 7,000  
and 15,000 bp, which I believe it was, that the speakers of this  
language were already highly variable or "mixed" if you like.

Regards,  
Paul Kekai Manansala

| 15|2002-06-06 00:50:30|chingdude56|Re: Pearic Peoples of Cambodia|

--- In austrian@y..., "pinatubo.geo" wrote:

> --- In austric@y..., "sferrarini" wrote:  
> > Dear Sirs,  
> >  
> > I write from Italy and i've a great interest for austroasiatic  
> > peoples. Particularly, in these last times i'm looking for recent  
> > news and images related to some small cambodian ethnic groups  
> > belonging to the Pearic branch of austroasiatic family.  
>  
>  
> Here's one image of the Pearr I was able to find on the web:  
>  
>  
> <http://angkor.com/pearr.jpg>

i think that is stefano's picture, actually. :)

"Call for info on Cambodian aboriginal groups

Stefano from Italy is looking for any recent news or photos concerning small Cambodian aboriginal groups located in remote mountainous areas. Particularly the "Kuy" and "Samr 𑜋 in the Dangrek chain, "Pearr" or "Porr" in the Cardamom chain, and "Saoch" in the Elephant chain. The only info he has found is a very old photo (1923) portraying three "Pearr" men (44KB). Does anyone have any recent photos of them, or any recent news about their lifestyle and integration with Khmer society? Email [angkor.com](mailto:angkor.com) and we will pass along the info to him." <http://www.angkor.com/news.shtml>

stefano, ethnologue's database lists many alternate names for these groups under the cambodian language section. also, khmer seem to apply the term "khmer leu" or "khmer loeu" (highland khmer) to all hill tribe minority groups. have you already attempted thorough searches with those terms?

[http://www.ethnologue.com/show\\_country.asp?name=Cambodia](http://www.ethnologue.com/show_country.asp?name=Cambodia)

an informative resource (in french):  
[http://www.refer.org/cbodg\\_ct/tur/tribus.htm](http://www.refer.org/cbodg_ct/tur/tribus.htm)

| 16|2002-06-06 09:42:51|frabrig|Re: Water buffalo domestication|

--- In austric@y..., "pinatubo.geo" wrote:

- > The date and place of water buffalo domestication may be relevant to
- > the current discussion.
- >
- > The dates for the swamp buffalo vary from 5000 to 2500 BCE.

Thank you for posting the abstract attached below re: the divergence of the swamp and river types of wild Asian buffalo from a common \*Bubalus arnee\*-like progenitor.

You add that the domestication of the swamp variety of buffalo took place -- evidently in (southern?) China, as the authors of the abstract posted by you maintain, and subsequently in the whole of Southeast Asia -- between the fifth and the third millennium BCE. These, I believe, are also regarded by the archaeologists as the term dates for the domestication of the river variety of buffalo in the regions of the Indian subcontinent where the Sarasvati-Sindhu civilization later flourished. I will post some extracts from a paper by R. Meadow supporting this datation.

The nature and function(s) of the sacrifices of buffaloes performed by the peoples of the SIVC are still a matter of debate. It is apparent from a number of SIVC seals that buffeloes were speared to death in some unknown religious ritual. In his *\_Deciphering the Indus Script\_* A. Parpola connects this hypothetical sacrificial activity with the cult of what he considers to be one of the paramount gods of the Harappans, the male buffalo-horned figure represented in some of the seals. Parpola looks at this figure as the supreme god of both the terrestrial and celestial waters, of life, death and fertility, and identifies him as one of the archetypes of the Vedic god Varuna through religious syncretism with Indo-European cult elements. He also connects this supposed Harappan supreme deity with the Vedic god of death Yama, whom he regards as a doublet of Varuna, and whose mythical mount is, as it is well-known, the water-buffalo. Parpola also discusses the possibility of an evolution of the Vedic royal horse-sacrifice (\*as'vamedha\*) from the buffalo-sacrifice tradition which, according to his views, might have been prevalent in the pre-Aryan SIVC (his argument is here mainly based on the appellation of the royal Vedic sacrificer's chief consort as \*mahis.I\*, i.e., buffalo-cow).

The topic of the discussion I am still attempting to start with the kind members of this forum is the possibility of a pre-historic cultural influence, coming from Southeast Asia, on the development of the above described religious components of the SIVC and Vedic

religions.

The point is: since the water-buffalo acts as the chief sacrificial animal in a class of structurally similar death and ancestor-worship ceremonies peculiar to a chain of archaic and sometimes isolated tribal cultures of India, Burma, Indo-China, southern China and Indonesia, and since this archaic sacrificial tradition is unknown to the Vedic funerary ritual (in which the typical animal offered as a sacrifice in order to honour the dead or the ancestors was, in case, the cow), could not it be that the association of both Yama and the (earlier?) Harappan divinity presiding -- if Parpola's thesis is accepted -- over death with the water-buffalo symbolism had its ultimate origin in a complex of funerary rites having developed in pre-historic times among the Austric-speaking peoples and their Tibeto-Burman, -, Tai-Kadai- and Miao-Yao-speaking neighbours in Southeast Asia?

If requested, I will post the list of all the tribes of India, Indo-China, southern China and Indonesia who sacrifice buffaloes in connection with funerary or ancestor-worship rituals. Since my list will be necessarily incomplete, I kindly invite all the list members to post here new evidence for this class of tribal mortuary sacrifices.

That is all for now.  
Kindest regards,

Francesco brighenti

- > An abstract from the latest research:
- >
- > Anim Genet 1998 Aug;29(4):253-64
- >
- > Genetic diversity of Asian water buffalo (*Bubalus bubalis*):
- > mitochondrial DNA D-loop and cytochrome b sequence variation.
- >
- > Lau CH, Drinkwater RD, Yusoff K, Tan SG, Hetzel DJ, Barker JS.
- >
- > Department of Biology, Universiti Putra Malaysia, Serdang, Malaysia.



>  
> Swamp and river buffalo mitochondrial DNA (mtDNA) was sequenced for  
> 303 bp of the cytochrome b gene for 54 animals from 14 populations,  
> and for 158 bp of the D-loop region for 80 animals from 11  
> populations. Only one cytochrome b haplotype was found in river  
> buffalo. Of the four haplotypes identified in swamp buffalo, one

found

> in all populations is apparently ancestral both to the other swamp  
> haplotypes and to the river haplotype. The phylogenetic

relationships

> among the 33 D-loop haplotypes, with a cluster of 11 found in swamp  
> buffalo only, also support the evolution of domesticated swamp and  
> river buffalo from an ancestral swamp-like animal, most likely  
> represented today by the wild Asian buffalo (*Bubalus arnee*). The

time

> of divergence of the swamp and river types, estimated from the D-

loop

> data, is 28,000 to 87,000 years ago. We hypothesise that the species  
> originated in mainland south-east Asia, and that it spread north to  
> China and west to the Indian subcontinent, where the river type

evolved

> and was domesticated. Following domestication in China, the  
> domesticated swamp buffalo spread through two separate routes,

through

> Taiwan and the Philippines to the eastern islands of Borneo and  
> Sulawesi, and south through mainland south-east Asia and then to the  
> western islands of Indonesia.

> --- In austric@y..., "pinatubo.geo" wrote:  
>  
>> The date and place of water buffalo domestication may be relevant to  
>> the current discussion.  
>>  
>> The dates for the swamp buffalo vary from 5000 to 2500 BCE.  
>  
> Thank you for posting the abstract attached below re: the divergence  
> of the swamp and river types of wild Asian buffalo from a common  
> \*Bubalus arnee\*-like progenitor.  
>  
> You add that the domestication of the swamp variety of buffalo took  
> place -- evidently in (southern?) China,

It must be in tropical areas of China like Yunnan, since  
it must be somewhere with the range of the wild water  
buffalo, an animal which only survives in dense tropical  
forests.

Of course, it could just as easily be anywhere in adjacent  
SE Asia.

> The nature and function(s) of the sacrifices of buffaloes performed  
> by the peoples of the SIVC are still a matter of debate. It is  
> apparent from a number of SIVC seals that buffeloes were speared to  
> death in some unknown religious ritual. In his \_Deciphering the Indus  
> Script\_ A. Parpola connects this hypothetical sacrificial activity  
> with the cult of what he considers to be one of the paramount gods of  
> the Harappans, the male buffalo-horned figure represented in some of  
> the seals. Parpola looks at this figure as the supreme god of both  
> the terrestrial and celestial waters, of life, death and fertility,  
> and identifies him as one of the archetypes of the Vedic god Varuna  
> through religious syncretism with Indo-European cult elements. He  
> also connects this supposed Harappan supreme deity with the Vedic god  
> of death Yama, whom he regards as a doublet of Varuna, and whose  
> mythical mount is, as it is well-known, the water-buffalo. Parpola  
> also discusses the possibility of an evolution of the Vedic royal  
> horse-sacrifice (\*as'vamedha\*) from the buffalo-sacrifice tradition  
> which, according to his views, might have been prevalent in the pre-  
> Aryan SIVC (his argument is here mainly based on the appellation of  
> the royal Vedic sacrificer's chief consort as \*mahis.I\*, i.e.,

> buffalo-cow).

>

> The topic of the discussion I am still attempting to start with the

> kind members of this forum is the possibility of a pre-historic

> cultural influence, coming from Southeast Asia, on the development of

> the above described religious components of the SIVC and Vedic

> religions.

>

> The point is: since the water-buffalo acts as the chief sacrificial

> animal in a class of structurally similar death and ancestor-worship

> ceremonies peculiar to a chain of archaic and sometimes isolated

> tribal cultures of India, Burma, Indo-China, southern China and

> Indonesia, and since this archaic sacrificial tradition is unknown to

> the Vedic funerary ritual (in which the typical animal offered as a

> sacrifice in order to honour the dead or the ancestors was, in case,

> the cow), could not it be that the association of both Yama and the

> (earlier?) Harappan divinity presiding -- if Parpola's thesis is

> accepted -- over death with the water-buffalo symbolism had its

> ultimate origin in a complex of funerary rites having developed in

> pre-historic times among the Austric-speaking peoples and their

> Tibeto-Burman, -, Tai-Kadai- and Miao-Yao-speaking neighbours in

> Southeast Asia?

>

As you note, cattle takes the place of the buffalo among some of the peoples mentioned, so that could also be the case in the Vedic ritual

If the miniature sculpture from Ban Chiang and other early SE Asian sites really have religious significance, as some have theorized, then both the water buffalo and zebu were sacred animals. They could have been interchangeable in the sacrifice at one time.

> If requested, I will post the list of all the tribes of India, Indo-

> China, southern China and Indonesia who sacrifice buffaloes in

> connection with funerary or ancestor-worship rituals.

I, for one, would like to see this list as well as any instances of cattle sacrifice that involve funerary/ancestor

worship rituals.

Many of the highland tribes of Vietnam and Laos sacrifice cattle at funerals. This, of course, would bring into question Paul Benedict's Austro-Thai theory and what relation it has to the Austric family of languages.

Regards,  
Paul Kekai Manansala

| 18|2002-06-06 18:56:20|philippinestudies|Invitation to network with  
<http://groups.yahoo.com/group/Alibata/>

austric yahoogroup:

ABSN, Ancient Baybayin Scripts Network, has invited you and your forum for an open forum exchange and discussion on the ancient writing scripts of the Philippines. Get diverse perspectives on news and updates on the current situation of Baybayin(Alibata)scripts of today. Meet fellow people interested in the ancient art of writing Baybayin in the many languages of the Philippines. Alibata, as known by many, is a popular topic that can range from tattoos, art, fashion design, computer fonts, Philippine Studies, and indigenous bridges that bonds all peoples to the natural beauty of Philippines. We welcome academic conversations in Pre-Hispanic Philippine Literature, Philippine Pre-History, Analysis on Extinct, Living, and Endangered Scripts of the Philippines, different aspects of Ancient Philippines, Austronesian Linguistics, and other historical topics that you may want to share with the network group. The discussion about current and past Philippine educational programs, constitutional law, regulations, political policies that affect the greater understanding of Baybayin/Alibata Scripts is always accepted at ABSN, Ancient Baybayin Scripts Network.

Be part of our Alibata/Baybayin Discussions and diverse Philippine History Conversations with the ABSN:  
<http://groups.yahoo.com/group/Alibata/>

| 19|2002-06-16 07:31:51|frabrig|Re: Water-buffalo sacrifice in Southeast Asia and India|

Dear Paul,

Sorry for the delay in posting this message. I was out of town for some days.

The following is my promised compendium of ethnographical data

documenting that the water-buffalo, along with other horned cattle such as the mithun and the zebu, is the most important sacrificial animal being traditionally offered at death and ancestor-worship ceremonies by a large number of tribal groups or ethnic minorities throughout tropical Asia.

Please take it as a basis for further discussion. I am particularly interested in comparing more sources with my data. If you have access to more pieces of information regarding the interrelationship of buffalo-sacrifice and funerary rituals in Southeast Asia, please post them here. As you can easily argue, I am still searching for data justifying a possible common origin for all of these funerary rituals associated with the sacrifice of bovines. Did these ritual traditions originate out of the Vedic religious culture (see the symbolical connection between Yama and the water-buffalo or the sacrifice of cows at Vedic funerals) or are they older than the Veda itself?

---

#### DRAVIDIAN TRIBES OF THE NILGIRI HILLS (SOUTH INDIA)

\*Toda ? Buffalo is sacrificed at both primary and secondary funerals. The animal is pursued, dragged by the horns, and finally killed with an axe stroke (there is no bloodshed). The Todas do not eat the sacrificed buffaloes' meat. There is a ritual contact between the dead buffalo's horns and the departed person's corpse (at primary funerals) or the mourners' hands (at secondary funerals). The latter features may be related to the similar usages adopted at ancient Vedic funerals with the sacred cows termed as \*anustarani\* and \*vaitarani\* respectively.

\*Kota ? Mortuary oblations of buffaloes performed in a fashion similar to the Todas'. Main difference: the Kotas, unlike the Todas, ritually eat the meat of the sacrificed buffaloes.

#### MUNDA AND DRAVIDIAN TRIBES OF CENTRAL-EASTERN INDIA

\*Gadaba (tribe of Orissa divided in two branches, one speaking a Munda language, the other a Dravidian one) ? The buffalo is sacrificed at both primary and secondary ceremonies for the disposal of the dead. The secondary ceremony, memorial in character, is a grand affair involving the slaughtering of tens and tens of buffaloes (also of cows). This has a direct bearing on the increase of social status, hierarchy of kinship ties, reciprocal donations of buffaloes, etc. Dedication of megalithic monuments (menhirs and stone slabs).

Association of each sacrificial buffalo with a branch of silk-cotton tree (*Bombax malabaricum* or *Salmalia malabarica*). The donors of the memorial feast cannot eat the meat of the sacrificed animals. Buffaloes are believed to carry off the souls of the departed and to become their property in the afterworld, thus increasing their relative status in comparison with that of other ancestral spirits.

\*Hill Saora (Munda-speaking tribe of Orissa) ? Primary and secondary ceremonies for the disposal of the dead similar to those of their neighbours the Gadabas, with yet much more integration of shamanistic practices (mainly carried out by female practitioners). The secondary funeral ceremony involved in the past the butchering of hundreds of buffaloes.

\*Bonda (Munda-speaking tribe of Orissa) ? Primary and secondary ceremonies for the disposal of the dead pivoted upon zebu- and pig-sacrifices. Sometimes buffaloes too are offered. Erection of dolmen-like memorial stones beside which branches of the silk-cotton tree are planted.

\*Kondh (group of Dravidian-speaking tribes of Orissa) ? All hill-Kondh tribes use to sacrifice buffaloes at funerals. They assign the buffalo the role of supernatural soul-carrier. In the past there were elaborate and warlike funeral dances performed by dancers wearing horned headgears. Many Kondh communities also observe an annual ceremony of ancestor-worship centred round the sacrifice of a zebu by each household. Inside many Kondh traditional houses there are still now horned posts showing nice carved designs (clan marks), which are worshipped as symbols of the ancestors.

\*Bison-horn Maria, Hill Maria, Muria (Dravidian-speaking tribes of the Bastar district of Chattisgarh all belonging to the Gond ethnic group) ? Sacrifices, performed without bloodshed, of zebu cows at cremation or burial ceremonies associated with the erection of megalithic monuments such as menhirs, carved memorial poles or forked (Y-shaped) posts, with the latter being clearly modelled on the shape of bovine horns. The cow's tail is affixed on the top of such monuments.

\*Koya (Dravidian-speaking tribe of southern Orissa and northern Andhra Pradesh culturally and linguistically related to the Gonds) ? Funerary observances nearly identical to those of the Gonds of Bastar, including cow-sacrifice.

\*Raj Gond of Adilabad (Dravidian-speaking tribe of Andhra Pradesh belonging to the Gond ethnic group) ? They sacrifice zebu cows at

both primary and secondary ceremonies for the disposal of the dead. On the occasion of the secondary ceremony the truncated head of the animal is placed at the foot of a forked post, while its tail, liver, lungs and heart are affixed to the summit of this post.

#### CHOTA NAGPUR PLATEAU AND ADJOINING AREAS

\*Munda of the Ranchi Plateau ? Only one important source (W. Koppers) mentions a ritual custom of theirs consisting in sacrificing a buffalo on the occasion of the ceremony of the secondary burial of the bodily remains of the deceased under his/her own family's megalithic tomb. Other sources only mention the sacrifice of an ox, and still others, none of the two. The animal is killed without bloodshed. The mouth, ears and hooves of the dead animal are buried in the courtyard of the house of the deceased.

\*Santal (Munda-speakers) ? They sacrifice a zebu cow during their secondary funerals. The oblation is preceded by divination rites accompanied by spirit-possession phenomena.

\*Sauriya Pahariya of the Rajmahal Hills (Dravidian-speakers) ? Sacrifices of zebu cows at both primary and secondary mortuary ceremonies.

#### KHASI AND GARO HILLS (MEGHALAYA)

\*Khasi (Mon-Khmer-speakers) ? Very much elaborate funerary ritual characterized by the periodic displacement of the bodily remains of the deceased to a series of different megalithic monuments until they find their final resting place inside a matrilineal clan's ossuary. This ritual process is accompanied at each stage by sacrifices of zebras, which are killed without bloodshed. The horns and the jawbones of the sacrificed animals are customarily fixed to posts placed over the menhirs that are erected at different stages of the Khasi funerary ritual.

\*Garo (Tibeto-Burman-speakers) ? They too sacrifice zebras at funerals.

#### INDO-BURMESE BORDER REGION

\*Naga tribes (Tibeto-Burman-speakers) ? A massive sacrificial activity ? the bovine victims alternatively immolated are the mithun, the buffalo, and the zebu ? associated with the erection of megalithic monuments and of forked poles (often of gigantic size) marks their well-known Feasts of Merit. The latter are not directly connected with funerary ceremonies, but yet there are cases (such as,

for instance, among the Angami Nagas) in which they are celebrated to honour the donor's departed father or another eminent dead relative of his. Thus, there appears to be some structural similarities between the megalithic Feasts of Merit celebrated by the Angami Nagas and the megalithic funerary ceremonies of the memorial class observed by other ethnic groups of eastern India. While the latter are organized to commemorate the dead and/or the ancestors, the former are organized to send to posterity the memory of a living man, usually a powerful one. Moreover, the Sema Nagas are reported to sacrifice oxen and pigs at the funeral of a warrior and then expose on a wooden rack the skulls of the sacrificed animals along with those of the animals killed during the Feasts of Merit offered by the deceased during his lifetime. It is believed that the souls of all of these animals will follow the dead to the afterworld. This is apparently more in line with the tribal funerary rituals described so far and with those, still to be described, prevailing among some ethnic groups of Indonesia. The technique adopted by most of the Naga tribes to put the sacrificial bovines to death, namely, the spearing technique, is also largely prevalent among the buffalo-sacrificing tribes of Indo-China. Finally, we can also find among the Naga tribes an ubiquitous symbolic use of the motif of the mithun's horns or head in art (on village carved doors and drum-gongs), architecture (the V-shaped finials at either edge of the roof ridge of the chiefs' houses), the paraphernalia of sacred dance (e.g., horned head-hunting trophies) and the implements of sacrifice (the huge, Y-shaped sacrificial poles, often with a mithun-head motif carved on them, which resemble the forked poles with a buffalo-head carved on them used by some Desia Kondh communities of Orissa in connection with buffalo-sacrifice). Items of religious culture nearly identical to these are also noticed among some non-Islamized ethnic groups of Indonesia, e.g. among the Torajas.

\*Kuki/Chin tribes (Tibeto-Burman-speakers) ? Most of the Indian and Burmese tribes speaking languages belonging to the Kuki/Chin group immolate bovines at funerals. The Kukis of Manipur butcher mithuns, buffaloes, cows, and also horses, pigs, goats and dogs, during the grandiose funerary feasts celebrated to honour each dead tribal chief. They subsequently place the heads of the slain animals under the chief's smoked and hardened corpse to symbolize his ownership over these animals in the afterworld. The Lushais of Mizoram sacrifice mithuns, and also pigs, goats and dogs, to provide meat for their funeral feast, and believe that the souls of the sacrificed animals will accompany the dead to his final abode in the afterworld. In the case of a chief's funeral, the skulls of the sacrificed animals, and particularly those of mithuns, are exposed on the stone slab, often associated with a menhir, that commemorates the deceased



person. In ordinary Lushai burials the sacrificed mithun's skull is placed above a pole planted near the tomb. The Lakhers of Mizoram observe funerary rites nearly identical to those observed by the Lushais. The Chin tribes of Burma immolate mithuns on the occasion of their funeral ceremonies. Before the sacrifice the animals are tied up to forked sacrificial posts.

#### NORTHERN INDO-CHINA AND SOUTH CHINA

\*Wa (Mon-Khmer-speaking tribe of the Burma-Yunnan border region) ? Sacrifices of buffaloes, oxen and pigs made in different ritual contexts, including the funerary one. The animals are speared to death after the common practice prevalent in the regions extending from Assam to Vietnam. The Wa sacrificial activity, directed by shamans-diviners, revolves round the village's sacred wooden drum, venerated in the so-called House of Spirits (a cult feature that is also found in the Naga religious culture). This drum is regarded as the abode of the tribe's Great Ancestress. The bones of the animals sacrificed to the Wa divinities and ancestral spirits are affixed to the beams of the drum-house. The periodic reinstallation of a newly-made sacred drum in the House of Spirits is an important religious ceremony that culminates in the sacrifice of a bull, previously tied to a Y-shaped post. Y-shaped posts, planted in rows in front of the village houses, also commemorate the householders' offerings of buffaloes to divinities and ancestral spirits. These poles are sometimes of imposing proportions like those erected by the Nagas during their Feasts of Merit. The skulls of the sacrificed buffaloes are preserved in a heap lying in the house, or are affixed to the inner house walls. In the chiefs' longhouses the symbolism of buffalo-horns is also present in the V-shaped finials surmounting the edges of the roof ridge (a feature which is also found in the Assam-Burma border region and in the whole of Indonesia). The reconstruction of the chiefs' longhouses is conceived as a ritual of death and rebirth (of the house itself), and is thus preceded by sacrifices of buffaloes, oxen and pigs on the analogy of the actual funerary ritual. The latter religious feature is also present in parts of Indonesia, e.g. among the Acehese.

\*Lamet (Mon-Khmer-speaking tribe of northern Laos) ? They sacrifice buffaloes to honour their ancestral spirits. The skulls of the slain animals are hung over the altar used to deposit the offerings to the ancestors or are affixed to the front wall of the donor's house.

\*Jingpo (Tibeto-Burman-speaking tribe settled in the Indo-Burmese-Chinese border region and grouped along with its cognate tribes in the so-called Kachin people) ? They sacrifice buffaloes at funerals.

The animals are, also in this case, tied up to Y-shaped sacrificial posts. The horns of the sacrificed buffaloes are affixed to the bamboo huts covered with cloth that are temporarily built upon the earth mounds under which the Jingpos use to bury their dead.

\*Akha/Hani (Tibeto-Burman tribe speaking a language belonging to the Yi/Lolo sub-family and spread over vast areas of mainland Southeast Asia which include southern Yunnan and the northern provinces of Burma, Thailand, Laos and Vietnam) ? Buffalo-sacrifice forms an important part of their funerary ritual. The carcasses of the slain animals are laid out on the ground, their heads covered with uncooked rice heaps, and form, so to say, a supernatural audience for the recital, performed by the dead person's eldest son or by a professional bard, of the countless names of the ancestors of the departed, who are thought to be waiting for him in the afterworld. The typical Akha/Hani coffin is shaped as a boat and is adorned on the top by two bent protuberances resembling the profile of a pair of buffalo-horns.

\*Miao/Hmong tribes (Sinitic-speaking tribes whose languages belong to the Miao-Yao sub-family, and who are spread over southern China, where they are known as Miao, and north-eastern Indo-China, where they are known as Hmong) ? The Hmong highlanders of North Vietnam sacrifice a buffalo, or alternatively an ox, at their funerals. Buffaloes are offered as a sacrifice on the occasion of funerals by the Miaos of Sichuan too. The Long-Horn Miaos of Guizhou, on the contrary, only sacrifice cows at their funerals. Moreover, the Miao tribes of Guizhou celebrate, generally every thirteen years, a great buffalo-sacrifice festival meant to celebrate a series of good crops and, at one time, to honour the ancestors. The buffalo may in some cases be replaced by a bull or an ox. The donor's family members take away the head of the slain animal, which is fixed on the top of a ceremonial post. The horns of the sacrificed bovines are later on heaped up in a special room of the house dedicated to ancestor-worship. Generally speaking, all the Miao/Hmong tribes conceive bovines as the best suited animal symbols for their ancestral heroes. The myths of the Miaos of Guizhou preserve the memory of legendary buffalo-sacrifices offered in by-gone days by the householders to the ancestral spirits. In this class of myths the ancestors are represented by a large sacred wooden drum identified as the Great Ancestor. These mythological features appear to share a common background with the Wa religious tradition about the sacred drum and its association with the ancestral spirits and the sacrifice of bovines dedicated to them.

\*Black Tai (Tai-Kadai-speaking ethnic group of northern Vietnam and

Laos) ? They sacrifice buffaloes, cows or pigs at funerals. Portions of meat are then offered to the spirit of the departed before the burial ceremony. Once in a year, after the rice harvest, buffalo-meat is offered to the ancestral spirits in the room of the house dedicated to ancestor-worship.

\*Dai (Tai-Kadai-speaking ethnic group of southern Yunnan) ? They sacrifice buffaloes at funerals, soon after the burial ceremony. The heads of the sacrificed buffaloes are affixed to the bamboo hut in which the offerings to the departed are deposited. The horns of the animals are later on affixed to the inner walls of the house of the deceased.

\*Zhuang (the largest Tai-Kadai-speaking ethnic minority of China, peopling the autonomous province of Guangxi) ? They honour and propitiate their ancestors with sacrifices of buffaloes.

#### SO-CALLED MONTAGNARDS OF THE ANNAMITE PLATEAU

\*Jarai (Austronesian-speakers) ? They sacrifice buffaloes on a mass scale during funeral ceremonies. Each dead person is buried under a profusely decorated hut burial of his own, which is surrounded by a fence having wooden sculptures fixed on it. Outside the fence, at the time of celebration of the funerary rites, many forked (Y-shaped or V-shaped) poles are planted, which are used to tie the sacrificial buffaloes up. The buffaloes are ?- if I am not wrong ? generally speared to death. The heads and hooves of the slain animals are finally nailed to the hut burial.

\*Bahnar (Mon-Khmer-speakers) ? They reportedly observe a funerary ritual which is nearly identical to that observed by their Austronesian-speaking Jarai neighbours, but it is still not clear to me as to whether they, like the latter, use to offer buffaloes in sacrifice on a mass scale or not.

\*Mnong (Mon-Khmer-speaking tribe also found in Cambodia) ? Their burials, like the Jarais', mainly consist of hut-like structures. No information is available to me as to their eventual performance of buffalo-sacrifices at funerals. Yet, it is known to me that they, as a norm, place the corpse of each dead person in a buffalo-shaped wooden coffin, which is thereafter installed in the hut burial.

\*Gie Trieng (Mon-Khmer speakers) ? They place their dead in boat-shaped wooden coffins on which they use to carve some buffalo figures. Similarly, their typical burial monument is surmounted by a buffalo-head carved out of wood. No information available as to

whether they perform buffalo-sacrifices at funerals.

## INDONESIA

Different Austronesian-speaking ethnic groups of Indonesia regard the buffalo as the soul-carrier par excellence and consider it the most important animal they can offer as a sacrifice at both primary and secondary ceremonies for the disposal of the dead, as well as at ancestor-worship ceremonies. The identification of a clan's common mansion with a celestial buffalo, the main symbol of the clan's ancestral spirits taken as a whole, is very commonly found in Indonesia. This identification is reflected in the curved shape of the roof ridge (Minangkabau, Batak, Toraja), suggestive of the profile of the buffalo's horns or of that of a boat ?- is that the "material" boat by which the ancestors reached the islands of Indonesia, or is that some kind of a mythological "buffalo-boat" by which the ancestors were carried off to the afterworld? This kind of curved roof is already depicted on some Dong-Son bronze drums from Vietnam (c. 500 BC ? 100 AD). The identification of the clan's house with a buffalo is similarly reflected in the V-shaped finials rising at either edge of the roof ridge of the houses built by some Indonesian tribes. This architectural feature, shared in by some Naga and Kuki/Chin tribes as well as by the Wa tribe, already figures in a bronze model house from Yunnan dated to c. 300 BC. In many an Indonesian language such V-shaped roof finials are generally termed as "horns", with this suggesting that they actually symbolize buffalo-horns. Also the buffalo-heads made out of wood or straw, which often adorn the front porch of the traditional houses built by some Toraja and Batak tribes, aim at identifying the clan's house with a buffalo, a hypostasis of the ancestral spirits.

\*Dayak tribes ? The Tunjung and Benuaq Dayaks of eastern Kalimantan observe, after the harvest, a seasonal ceremony of secondary burial of the bones of the dead on the occasion of which the central rite is represented by the sacrifice of buffaloes. The Ngaju Dayaks of central Kalimantan observe a similar, yet more complex mortuary ritual which is also pivoted upon the sacrifice of buffaloes. The animals are speared to death by the kinsmen of the departed. The heads of all the slain buffaloes are exposed on a special wooden structure (resembling an analogous sacrificial rack commonly used by the Naga tribes at funeral ceremonies). When the ceremony of secondary burial is concluded, the jawbones of the sacrificed buffaloes are affixed to the wooden ossuary containing the ashes of all the deceased members of a single lineage. It is maintained by scholars that buffalo-sacrifice replaced human sacrifice in the mortuary ritual of the Dayak tribes as late as the 19th century. This appears to be the case with the Toraja tribes of Sulawesi as well.

\*Toraja tribes ? Grandiose secondary funeral ceremonies usually held after the harvest and centred round the mass sacrifice of buffaloes. In past times the Toraja mortuary ritual also contemplated the sacrifice of human beings (slaves). In Toraja cosmogonic myths the buffalo is described as the mystic brother of the first human being. Sacrifices of buffaloes accompany all stages of the Toraja ritual for the disposal of the dead, culminating in the ceremony of the secondary funeral. Along with the souls of pigs, the souls of buffaloes are believed to accompany the dead in his journey to the afterworld. There is a complex system of donation of sacrificial buffaloes by the kinsmen of the departed which, with its underlying social obligation norms, resembles the system in vogue among some Orissan tribes (for instance the Gadabas) as well as that in vogue at the Feasts of Merit celebrated by the Naga tribes and by some Indonesian ethnic groups other than the Torajas. The number of buffaloes donated by a family conditions the social recognition of the kinship status of the donors. The total number of sacrificial buffaloes determines the status of the departed soul in the afterworld. In times past these funerary sacrifices were associated with the erection of megaliths. The buffalo's throat is cut with the use of a short sword, but in the past the animal used to be speared to death as per the custom generally prevailing in Southeast Asia, Assam included. The horns of the slain buffaloes are affixed to the front pole sustaining the roof of the traditional Toraja house, which is curvilinear as a pair of buffalo-horns. Also the wooden coffins used at funerals, which are shaped as miniature Toraja houses, have an identical curvilinear roof. Toraja houses and tombs also feature images in bas-relief carved out of wood that depict buffalo heads or buffalo-mounted ancestors.

\*Tribes of Sumba ? The famous megalithic tombs at Sumba are richly decorated with archaic sculptures in bas-relief that often represent the head or the horns of a buffalo. This is because the rites of secondary burial of the bones of the deceased Sumbanese nobles, for which the tombs are erected, are pivoted upon the mass sacrifice of buffaloes and other animals, among which the horse is also prominent and represented in the sculptures adorning the tombs. Also in this case, as in that of the Torajas, the Jarais of South Vietnam, and the far more distant tribal groups of Orissa respectively, the more buffaloes are offered in sacrifice, the more elevated is the status putatively reached by the deceased in the afterworld. The buffaloes are killed cutting their neck with a short sword or a knife. As among the Torajas and Dayaks, the ceremony of secondary burial was once accompanied by the ritual murdering of slaves. Many traditional houses at Sumba have their outer walls covered with the horns of the

buffaloes sacrificed at secondary death ceremonies. These horn-trophies are disposed in vertical rows as is also common among the Torajas. Buffaloes are also sacrificed by some Sumbanese ethnic groups, for instance the Weyewas, in the ambit of ancestor-worship rites. The Laboya ethnic group of Sumba conceive their houses as living buffaloes, identifying them with their ancestors.

\*Tribes of Flores ? The religion of the Hoga Sara ethnic group of Flores is centred round a form of ancestor-cult that requires the annual renewal or the fresh installation of a series of family ritual objects (sacrificial and memorial posts, stone altars used to make offerings to the ancestors, miniature sacred huts dedicated to the worship of ancestresses). This annual ceremony involves the sacrifice of a buffalo and the aspersion of all the newly-installed or regenerated ancestor-cult objects with its blood in order to allow the non-placated ancestral spirits to take up their final abode in such objects. The buffalo, previously tied up to a forked pole, is killed by the village headman with a ceremonial knife.

#### IGOROT TRIBES OF THE PHILIPPINES

See message no. 6 posted by me in this list.

Regards,  
Francesco Brighenti

| 20|2002-06-16 09:11:20|a.manansala@attbi.com|Re: Water-buffalo sacrifice in Southeast Asia and India|

Thank you, Francesco for the extensive listing.

I guess we might also mention something that we have discussed in private email. The possible connection of the buffalo sacrifice with the royal asvamedha sacrifice of the Vedas.

The primary queen is known as mahisi, or water buffalo cow, in the asvamedha ritual.

Royal water buffalo sacrifices still take place in South India. I have also heard of traditions in Southeast Asia where the tribal chief must take part in buffalo sacrifices.

Do you have information on the use of the bovine sacrifice among royalty or chieftancies in the same

region?

Regards,  
Paul Kekai Manansala

> Dear Paul,

>

> Sorry for the delay in posting this message. I was out of town for  
> some days.

>

> The following is my promised compendium of ethnographical data  
> documenting that the water-buffalo, along with other horned cattle  
> such as the mithun and the zebu, is the most important sacrificial  
> animal being traditionally offered at death and ancestor-worship  
> ceremonies by a large number of tribal groups or ethnic minorities  
> throughout tropical Asia.

>

> Please take it as a basis for further discussion. I am particularly  
> interested in comparing more sources with my data. If you have access  
> to more pieces of information regarding the interrelationship of  
> buffalo-sacrifice and funerary rituals in Southeast Asia, please post  
> them here. As you can easily argue, I am still searching for data  
> justifying a possible common origin for all of these funerary rituals  
> associated with the sacrifice of bovines. Did these ritual traditions  
> originate out of the Vedic religious culture (see the symbolical  
> connection between Yama and the water-buffalo or the sacrifice of  
> cows at Vedic funerals) or are they older than the Veda itself?

>

>

> -----

>

> **DRAVIDIAN TRIBES OF THE NILGIRI HILLS (SOUTH INDIA)**

>

> \*Toda ? Buffalo is sacrificed at both primary and secondary funerals.  
> The animal is pursued, dragged by the horns, and finally killed with  
> an axe stroke (there is no bloodshed). The Todas do not eat the  
> sacrificed buffaloes' meat. There is a ritual contact between the  
> dead buffalo's horns and the departed person's corpse (at primary  
> funerals) or the mourners' hands (at secondary funerals). The latter  
> features may be related to the similar usages adopted at ancient  
> Vedic funerals with the sacred cows termed as \*anustarani\* and  
> \*vaitarani\* respectively.

>

> \*Kota ? Mortuary oblations of buffaloes performed in a fashion  
> similar to the Todas'. Main difference: the Kotas, unlike the Todas,  
> ritually eat the meat of the sacrificed buffaloes.

>  
> **MUNDA AND DRAVIDIAN TRIBES OF CENTRAL-EASTERN INDIA**

>  
> \*Gadaba (tribe of Orissa divided in two branches, one speaking a  
> Munda language, the other a Dravidian one) ? The buffalo is  
> sacrificed at both primary and secondary ceremonies for the disposal  
> of the dead. The secondary ceremony, memorial in character, is a  
> grand affair involving the slaughtering of tens and tens of buffaloes  
> (also of cows). This has a direct bearing on the increase of social  
> status, hierarchy of kinship ties, reciprocal donations of buffaloes,  
> etc. Dedication of megalithic monuments (menhirs and stone slabs).  
> Association of each sacrificial buffalo with a branch of silk-cotton  
> tree (*Bombax malabaricum* or *Salmalia malabarica*). The donors of the  
> memorial feast cannot eat the meat of the sacrificed animals.  
> Buffaloes are believed to carry off the souls of the departed and to  
> become their property in the afterworld, thus increasing their  
> relative status in comparison with that of other ancestral spirits.

>  
> \*Hill Saora (Munda-speaking tribe of Orissa) ? Primary and secondary  
> ceremonies for the disposal of the dead similar to those of their  
> neighbours the Gadabas, with yet much more integration of shamanistic  
> practices (mainly carried out by female practitioners). The secondary  
> funeral ceremony involved in the past the butchering of hundreds of  
> buffaloes.

>  
> \*Bonda (Munda-speaking tribe of Orissa) ? Primary and secondary  
> ceremonies for the disposal of the dead pivoted upon zebu- and pig-  
> sacrifices. Sometimes buffaloes too are offered. Erection of dolmen-  
> like memorial stones beside which branches of the silk-cotton tree  
> are planted.

>  
> \*Kondh (group of Dravidian-speaking tribes of Orissa) ? All hill-  
> Kondh tribes use to sacrifice buffaloes at funerals. They assign the  
> buffalo the role of supernatural soul-carrier. In the past there were  
> elaborate and warlike funeral dances performed by dancers wearing  
> horned headgears. Many Kondh communities also observe an annual  
> ceremony of ancestor-worship centred round the sacrifice of a zebu by  
> each household. Inside many Kondh traditional houses there are still  
> now horned posts showing nice carved designs (clan marks), which are  
> worshipped as symbols of the ancestors.

>  
> \*Bison-horn Maria, Hill Maria, Muria (Dravidian-speaking tribes of  
> the Bastar district of Chattisgarh all belonging to the Gond ethnic  
> group) ? Sacrifices, performed without bloodshed, of zebu cows at  
> cremation or burial ceremonies associated with the erection of  
> megalithic monuments such as menhirs, carved memorial poles or forked



> (Y-shaped) posts, with the latter being clearly modelled on the shape  
> of bovine horns. The cow's tail is affixed on the top of such  
> monuments.

>

> \*Koya (Dravidian-speaking tribe of southern Orissa and northern  
> Andhra Pradesh culturally and linguistically related to the Gonds) ?  
> Funerary observances nearly identical to those of the Gonds of  
> Bastar, including cow-sacrifice.

>

> \*Raj Gond of Adilabad (Dravidian-speaking tribe of Andhra Pradesh  
> belonging to the Gond ethnic group) ? They sacrifice zebu cows at  
> both primary and secondary ceremonies for the disposal of the dead.  
> On the occasion of the secondary ceremony the truncated head of the  
> animal is placed at the foot of a forked post, while its tail, liver,  
> lungs and heart are affixed to the summit of this post.

>

> CHOTA NAGPUR PLATEAU AND ADJOINING AREAS

>

> \*Munda of the Ranchi Plateau ? Only one important source (W. Koppers)  
> mentions a ritual custom of theirs consisting in sacrificing a  
> buffalo on the occasion of the ceremony of the secondary burial of  
> the bodily remains of the deceased under his/her own family's  
> megalithic tomb. Other sources only mention the sacrifice of an ox,  
> and still others, none of the two. The animal is killed without  
> bloodshed. The mouth, ears and hooves of the dead animal are buried  
> in the courtyard of the house of the deceased.

>

> \*Santal (Munda-speakers) ? They sacrifice a zebu cow during their  
> secondary funerals. The oblation is preceded by divination rites  
> accompanied by spirit-possession phenomena.

>

> \*Sauriya Pahariya of the Rajmahal Hills (Dravidian-speakers) ?  
> Sacrifices of zebu cows at both primary and secondary mortuary  
> ceremonies.

>

> KHASI AND GARO HILLS (MEGHALAYA)

>

> \*Khasi (Mon-Khmer-speakers) ? Very much elaborate funerary ritual  
> characterized by the periodic displacement of the bodily remains of  
> the deceased to a series of different megalithic monuments until they  
> find their final resting place inside a matrilineal clan's ossuary.  
> This ritual process is accompanied at each stage by sacrifices of  
> zebras, which are killed without bloodshed. The horns and the jawbones  
> of the sacrificed animals are customarily fixed to posts placed over  
> the menhirs that are erected at different stages of the Khasi  
> funerary ritual.

>  
> \*Garo (Tibeto-Burman-speakers) ? They too sacrifice zebus at funerals.

>  
> INDO-BURMESE BORDER REGION

>  
> \*Naga tribes (Tibeto-Burman-speakers) ? A massive sacrificial  
> activity ? the bovine victims alternatively immolated are the mithun,  
> the buffalo, and the zebu ? associated with the erection of  
> megalithic monuments and of forked poles (often of gigantic size)  
> marks their well-known Feasts of Merit. The latter are not directly  
> connected with funerary ceremonies, but yet there are cases (such as,  
> for instance, among the Angami Nagas) in which they are celebrated to  
> honour the donor's departed father or another eminent dead relative  
> of his. Thus, there appears to be some structural similarities  
> between the megalithic Feasts of Merit celebrated by the Angami Nagas  
> and the megalithic funerary ceremonies of the memorial class observed  
> by other ethnic groups of eastern India. While the latter are  
> organized to commemorate the dead and/or the ancestors, the former  
> are organized to send to posterity the memory of a living man,  
> usually a powerful one. Moreover, the Sema Nagas are reported to  
> sacrifice oxen and pigs at the funeral of a warrior and then expose  
> on a wooden rack the skulls of the sacrificed animals along with  
> those of the animals killed during the Feasts of Merit offered by the  
> deceased during his lifetime. It is believed that the souls of all of  
> these animals will follow the dead to the afterworld. This is  
> apparently more in line with the tribal funerary rituals described so  
> far and with those, still to be described, prevailing among some  
> ethnic groups of Indonesia. The technique adopted by most of the Naga  
> tribes to put the sacrificial bovines to death, namely, the spearing  
> technique, is also largely prevalent among the buffalo-sacrificing  
> tribes of Indo-China. Finally, we can also find among the Naga tribes  
> an ubiquitous symbolic use of the motif of the mithun's horns or head  
> in art (on village carved doors and drum-gongs), architecture (the V-  
> shaped finials at either edge of the roof ridge of the chiefs'  
> houses), the paraphernalia of sacred dance (e.g., horned head-hunting  
> trophies) and the implements of sacrifice (the huge, Y-shaped  
> sacrificial poles, often with a mithun-head motif carved on them,  
> which resemble the forked poles with a buffalo-head carved on them  
> used by some Desia Kondh communities of Orissa in connection with  
> buffalo-sacrifice). Items of religious culture nearly identical to  
> these are also noticed among some non-Islamized ethnic groups of  
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> Burmese tribes speaking languages belonging to the Kuki/Chin group  
> immolate bovines at funerals. The Kukis of Manipur butcher mithuns,

- > buffaloes, cows, and also horses, pigs, goats and dogs, during the
- > grandiose funerary feasts celebrated to honour each dead tribal
- > chief. They subsequently place the heads of the slain animals under
- > the chief's smoked and hardened corpse to symbolize his ownership
- > over these animals in the afterworld. The Lushais of Mizoram
- > sacrifice mithuns, and also pigs, goats and dogs, to provide meat for
- > their funeral feast, and believe that the souls of the sacrificed
- > animals will accompany the dead to his final abode in the afterworld.
- > In the case of a chief's funeral, the skulls of the sacrificed
- > animals, and particularly those of mithuns, are exposed on the stone
- > slab, often associated with a menhir, that commemorates the deceased
- > person. In ordinary Lushai burials the sacrificed mithun's skull is
- > placed above a pole planted near the tomb. The Lakhers of Mizoram
- > observe funerary rites nearly identical to those observed by the
- > Lushais. The Chin tribes of Burma immolate mithuns on the occasion of
- > their funeral ceremonies. Before the sacrifice the animals are tied
- > up to forked sacrificial posts.

#### > NORTHERN INDO-CHINA AND SOUTH CHINA

- > \*Wa (Mon-Khmer-speaking tribe of the Burma-Yunnan border region) ?
- > Sacrifices of buffaloes, oxen and pigs made in different ritual
- > contexts, including the funerary one. The animals are speared to
- > death after the common practice prevalent in the regions extending
- > from Assam to Vietnam. The Wa sacrificial activity, directed by
- > shamans-diviners, revolves round the village's sacred wooden drum,
- > venerated in the so-called House of Spirits (a cult feature that is
- > also found in the Naga religious culture). This drum is regarded as
- > the abode of the tribe's Great Ancestress. The bones of the animals
- > sacrificed to the Wa divinities and ancestral spirits are affixed to
- > the beams of the drum-house. The periodic reinstallation of a newly-
- > made sacred drum in the House of Spirits is an important religious
- > ceremony that culminates in the sacrifice of a bull, previously tied
- > to a Y-shaped post. Y-shaped posts, planted in rows in front of the
- > village houses, also commemorate the householders' offerings of
- > buffaloes to divinities and ancestral spirits. These poles are
- > sometimes of imposing proportions like those erected by the Nagas
- > during their Feasts of Merit. The skulls of the sacrificed buffaloes
- > are preserved in a heap lying in the house, or are affixed to the
- > inner house walls. In the chiefs' longhouses the symbolism of buffalo-
- > horns is also present in the V-shaped finials surmounting the edges
- > of the roof ridge (a feature which is also found in the Assam-Burma
- > border region and in the whole of Indonesia). The reconstruction of
- > the chiefs' longhouses is conceived as a ritual of death and rebirth
- > (of the house itself), and is thus preceded by sacrifices of
- > buffaloes, oxen and pigs on the analogy of the actual funerary

- > ritual. The latter religious feature is also present in parts of
- > Indonesia, e.g. among the Acehese.
- >
- > \*Lamet (Mon-Khmer-speaking tribe of northern Laos) ? They sacrifice
- > buffaloes to honour their ancestral spirits. The skulls of the slain
- > animals are hung over the altar used to deposit the offerings to the
- > ancestors or are affixed to the front wall of the donor's house.
- >
- > \*Jingpo (Tibeto-Burman-speaking tribe settled in the Indo-Burmese-
- > Chinese border region and grouped along with its cognate tribes in
- > the so-called Kachin people) ? They sacrifice buffaloes at funerals.
- > The animals are, also in this case, tied up to Y-shaped sacrificial
- > posts. The horns of the sacrificed buffaloes are affixed to the
- > bamboo huts covered with cloth that are temporarily built upon the
- > earth mounds under which the Jingpos use to bury their dead.
- >
- > \*Akha/Hani (Tibeto-Burman tribe speaking a language belonging to the
- > Yi/Lolo sub-family and spread over vast areas of mainland Southeast
- > Asia which include southern Yunnan and the northern provinces of
- > Burma, Thailand, Laos and Vietnam) ? Buffalo-sacrifice forms an
- > important part of their funerary ritual. The carcasses of the slain
- > animals are laid out on the ground, their heads covered with uncooked
- > rice heaps, and form, so to say, a supernatural audience for the
- > recital, performed by the dead person's eldest son or by a
- > professional bard, of the countless names of the ancestors of the
- > departed, who are thought to be waiting for him in the afterworld.
- > The typical Akha/Hani coffin is shaped as a boat and is adorned on
- > the top by two bent protuberances resembling the profile of a pair of
- > buffalo-horns.
- >
- > \*Miao/Hmong tribes (Sinitic-speaking tribes whose languages belong to
- > the Miao-Yao sub-family, and who are spread over southern China,
- > where they are known as Miao, and north-eastern Indo-China, where
- > they are known as Hmong) ? The Hmong highlanders of North Vietnam
- > sacrifice a buffalo, or alternatively an ox, at their funerals.
- > Buffaloes are offered as a sacrifice on the occasion of funerals by
- > the Miaos of Sichuan too. The Long-Horn Miaos of Guizhou, on the
- > contrary, only sacrifice cows at their funerals. Moreover, the Miao
- > tribes of Guizhou celebrate, generally every thirteen years, a great
- > buffalo-sacrifice festival meant to celebrate a series of good crops
- > and, at one time, to honour the ancestors. The buffalo may in some
- > cases be replaced by a bull or an ox. The donor's family members take
- > away the head of the slain animal, which is fixed on the top of a
- > ceremonial post. The horns of the sacrificed bovines are later on
- > heaped up in a special room of the house dedicated to ancestor-
- > worship. Generally speaking, all the Miao/Hmong tribes conceive

> bovines as the best suited animal symbols for their ancestral heroes.  
> The myths of the Miaos of Guizhou preserve the memory of legendary  
> buffalo-sacrifices offered in by-gone days by the householders to the  
> ancestral spirits. In this class of myths the ancestors are  
> represented by a large sacred wooden drum identified as the Great  
> Ancestor. These mythological features appear to share a common  
> background with the Wa religious tradition about the sacred drum and  
> its association with the ancestral spirits and the sacrifice of  
> bovines dedicated to them.

>

> \*Black Tai (Tai-Kadai-speaking ethnic group of northern Vietnam and  
> Laos) ? They sacrifice buffaloes, cows or pigs at funerals. Portions  
> of meat are then offered to the spirit of the departed before the  
> burial ceremony. Once in a year, after the rice harvest, buffalo-meat  
> is offered to the ancestral spirits in the room of the house  
> dedicated to ancestor-worship.

>

> \*Dai (Tai-Kadai-speaking ethnic group of southern Yunnan) ? They  
> sacrifice buffaloes at funerals, soon after the burial ceremony. The  
> heads of the sacrificed buffaloes are affixed to the bamboo hut in  
> which the offerings to the departed are deposited. The horns of the  
> animals are later on affixed to the inner walls of the house of the  
> deceased.

>

> \*Zhuang (the largest Tai-Kadai-speaking ethnic minority of China,  
> peopling the autonomous province of Guangxi) ? They honour and  
> propitiate their ancestors with sacrifices of buffaloes.

>

> SO-CALLED MONTAGNARDS OF THE ANNAMITE PLATEAU

>

> \*Jarai (Austronesian-speakers) ? They sacrifice buffaloes on a mass  
> scale during funeral ceremonies. Each dead person is buried under a  
> profusely decorated hut burial of his own, which is surrounded by a  
> fence having wooden sculptures fixed on it. Outside the fence, at the  
> time of celebration of the funerary rites, many forked (Y-shaped or V-  
> shaped) poles are planted, which are used to tie the sacrificial  
> buffaloes up. The buffaloes are ?- if I am not wrong ? generally  
> speared to death. The heads and hooves of the slain animals are  
> finally nailed to the hut burial.

>

> \*Bahnar (Mon-Khmer-speakers) ? They reportedly observe a funerary  
> ritual which is nearly identical to that observed by their  
> Austronesian-speaking Jarai neighbours, but it is still not clear to  
> me as to whether they, like the latter, use to offer buffaloes in  
> sacrifice on a mass scale or not.

>

> \*Mnong (Mon-Khmer-speaking tribe also found in Cambodia) ? Their  
> burials, like the Jarais', mainly consist of hut-like structures. No  
> information is available to me as to their eventual performance of  
> buffalo-sacrifices at funerals. Yet, it is known to me that they, as  
> a norm, place the corpse of each dead person in a buffalo-shaped  
> wooden coffin, which is thereafter installed in the hut burial.

>

> \*Gie Trieng (Mon-Khmer speakers) ? They place their dead in boat-  
> shaped wooden coffins on which they use to carve some buffalo  
> figures. Similarly, their typical burial monument is surmounted by a  
> buffalo-head carved out of wood. No information available as to  
> whether they perform buffalo-sacrifices at funerals.

>

> INDONESIA

> Different Austronesian-speaking ethnic groups of Indonesia regard the  
> buffalo as the soul-carrier par excellence and consider it the most  
> important animal they can offer as a sacrifice at both primary and  
> secondary ceremonies for the disposal of the dead, as well as at  
> ancestor-worship ceremonies. The identification of a clan's common  
> mansion with a celestial buffalo, the main symbol of the clan's  
> ancestral spirits taken as a whole, is very commonly found in  
> Indonesia. This identification is reflected in the curved shape of  
> the roof ridge (Minangkabau, Batak, Toraja), suggestive of the  
> profile of the buffalo's horns or of that of a boat ?- is that  
> the "material" boat by which the ancestors reached the islands of  
> Indonesia, or is that some kind of a mythological "buffalo-boat" by  
> which the ancestors were carried off to the afterworld? This kind of  
> curved roof is already depicted on some Dong-Son bronze drums from  
> Vietnam (c. 500 BC ? 100 AD). The identification of the clan's house  
> with a buffalo is similarly reflected in the V-shaped finials rising  
> at either edge of the roof ridge of the houses built by some  
> Indonesian tribes. This architectural feature, shared in by some Naga  
> and Kuki/Chin tribes as well as by the Wa tribe, already figures in a  
> bronze model house from Yunnan dated to c. 300 BC. In many an  
> Indonesian language such V-shaped roof finials are generally termed  
> as "horns", with this suggesting that they actually symbolize buffalo-  
> horns. Also the buffalo-heads made out of wood or straw, which often  
> adorn the front porch of the traditional houses built by some Toraja  
> and Batak tribes, aim at identifying the clan's house with a buffalo,  
> a hypostasis of the ancestral spirits.

>

> \*Dayak tribes ? The Tunjung and Benuaq Dayaks of eastern Kalimantan  
> observe, after the harvest, a seasonal ceremony of secondary burial  
> of the bones of the dead on the occasion of which the central rite is  
> represented by the sacrifice of buffaloes. The Ngaju Dayaks of  
> central Kalimantan observe a similar, yet more complex mortuary

> ritual which is also pivoted upon the sacrifice of buffaloes. The  
> animals are speared to death by the kinsmen of the departed. The  
> heads of all the slain buffaloes are exposed on a special wooden  
> structure (resembling an analogous sacrificial rack commonly used by  
> the Naga tribes at funeral ceremonies). When the ceremony of  
> secondary burial is concluded, the jawbones of the sacrificed  
> buffaloes are affixed to the wooden ossuary containing the ashes of  
> all the deceased members of a single lineage. It is maintained by  
> scholars that buffalo-sacrifice replaced human sacrifice in the  
> mortuary ritual of the Dayak tribes as late as the 19th century. This  
> appears to be the case with the Toraja tribes of Sulawesi as well.

>  
> \*Toraja tribes ? Grandiose secondary funeral ceremonies usually held  
> after the harvest and centred round the mass sacrifice of buffaloes.  
> In past times the Toraja mortuary ritual also contemplated the  
> sacrifice of human beings (slaves). In Toraja cosmogonic myths the  
> buffalo is described as the mystic brother of the first human being.  
> Sacrifices of buffaloes accompany all stages of the Toraja ritual for  
> the disposal of the dead, culminating in the ceremony of the  
> secondary funeral. Along with the souls of pigs, the souls of  
> buffaloes are believed to accompany the dead in his journey to the  
> afterworld. There is a complex system of donation of sacrificial  
> buffaloes by the kinsmen of the departed which, with its underlying  
> social obligation norms, resembles the system in vogue among some  
> Orissan tribes (for instance the Gadabas) as well as that in vogue at  
> the Feasts of Merit celebrated by the Naga tribes and by some  
> Indonesian ethnic groups other than the Torajas. The number of  
> buffaloes donated by a family conditions the social recognition of  
> the kinship status of the donors. The total number of sacrificial  
> buffaloes determines the status of the departed soul in the  
> afterworld. In times past these funerary sacrifices were associated  
> with the erection of megaliths. The buffalo's throat is cut with the  
> use of a short sword, but in the past the animal used to be speared  
> to death as per the custom generally prevailing in Southeast Asia,  
> Assam included. The horns of the slain buffaloes are affixed to the  
> front pole sustaining the roof of the traditional Toraja house, which  
> is curvilinear as a pair of buffalo-horns. Also the wooden coffins  
> used at funerals, which are shaped as miniature Toraja houses, have  
> an identical curvilinear roof. Toraja houses and tombs also feature  
> images in bas-relief carved out of wood that depict buffalo heads or  
> buffalo-mounted ancestors.

>  
> \*Tribes of Sumba ? The famous megalithic tombs at Sumba are richly  
> decorated with archaic sculptures in bas-relief that often represent  
> the head or the horns of a buffalo. This is because the rites of  
> secondary burial of the bones of the deceased Sumbanese nobles, for

> which the tombs are erected, are pivoted upon the mass sacrifice of  
> buffaloes and other animals, among which the horse is also prominent  
> and represented in the sculptures adorning the tombs. Also in this  
> case, as in that of the Torajas, the Jarais of South Vietnam, and the  
> far more distant tribal groups of Orissa respectively, the more  
> buffaloes are offered in sacrifice, the more elevated is the status  
> putatively reached by the deceased in the afterworld. The buffaloes  
> are killed cutting their neck with a short sword or a knife. As among  
> the Torajas and Dayaks, the ceremony of secondary burial was once  
> accompanied by the ritual murdering of slaves. Many traditional  
> houses at Sumba have their outer walls covered with the horns of the  
> buffaloes sacrificed at secondary death ceremonies. These horn-  
> trophies are disposed in vertical rows as is also common among the  
> Torajas. Buffaloes are also sacrificed by some Sumbanese ethnic  
> groups, for instance the Weyewas, in the ambit of ancestor-worship  
> rites. The Laboya ethnic group of Sumba conceive their houses as  
> living buffaloes, identifying them with their ancestors.

>  
> \*Tribes of Flores ? The religion of the Hoga Sara ethnic group of  
> Flores is centred round a form of ancestor-cult that requires the  
> annual renewal or the fresh installation of a series of family ritual  
> objects (sacrificial and memorial posts, stone altars used to make  
> offerings to the ancestors, miniature sacred huts dedicated to the  
> worship of ancestresses). This annual ceremony involves the sacrifice  
> of a buffalo and the aspersion of all the newly-installed or  
> regenerated ancestor-cult objects with its blood in order to allow  
> the non-placated ancestral spirits to take up their final abode in  
> such objects. The buffalo, previously tied up to a forked pole, is  
> killed by the village headman with a ceremonial knife.

>  
> IGOROT TRIBES OF THE PHILIPPINES

>  
> See message no. 6 posted by me in this list.

>  
> Regards,  
> Francesco Brighenti

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| 21|2002-06-21 10:58:23|frabrig|Re: Water-buffalo sacrifice in Southeast Asia and India|

--- In austric@y..., a.manansala@a... wrote:

> I guess we might also mention something that we have  
> discussed in private email. The possible connection of  
> the buffalo sacrifice with the royal asvamedha sacrifice  
> of the Vedas.  
> The primary queen is known as mahisi, or water buffalo  
> cow, in the asvamedha ritual.

This is a different sacrificial tradition, not necessarily related to pre-/proto-historic Austric cultures and the annexed mortuary rites, although I will be pleased to discuss this topic here with you and other contributors in a subsequent time.

> Royal water buffalo sacrifices still take place in  
> South India. I have also heard of traditions in  
> Southeast Asia where the tribal chief must take part in  
> buffalo sacrifices.

Not specifically in South India, rather in the whole of India! You are here referring to the Shakta-Tantric tradition of buffalo-sacrifice, which is yet still another topic.

> Do you have information on the use of the bovine  
> sacrifice among royalty or chieftancies in the same  
> region?

I will limit myself to indicate some instances of buffalo-sacrifices among royalty or chieftancies in Southeast Asia. As regards India, see the past offered by Hindu kings, while the buffalo-sacrifices offered by the Indian tribals in a funerary context are not connected with chieftainship).

To start with, you could read the following Web pages (sorry for the scantiness, but my research was not directly dealing with the buffalo-

sacrifices offered by kings or tribal chiefs of Southeast Asia inasmuch as I was mainly researching on FUNERARY sacrifices of bovines in those regions).

For the ancient buffalo-sacrifice festival of Aceh, on the occasion of which the sultan used to offer hundreds of animals, see the Web page:

[http://www.indonesianheritage.com/Encyclopedia/Early\\_Modern\\_History/Islam\\_And\\_Port\\_Sultans/Contests\\_And\\_Entertainment/contests\\_and\\_entertainment.html](http://www.indonesianheritage.com/Encyclopedia/Early_Modern_History/Islam_And_Port_Sultans/Contests_And_Entertainment/contests_and_entertainment.html)

For the sacrifices of buffaloes performed in diverse contexts by the sultans of the old princely States of the Malay peninsula, see R. O. Windstedt's online book reproduced at the following URL:

<http://www.sacred-texts.com/sha/sss.htm> (use the "Find" tool with "buffalo" as the key word).

As to the communal sacrifices of bovines performed by the Wa tribal chiefs, see the Web site:

<http://www.wa.peoples.org/Wa01.htm> (look for the page "Wa People Culture" and go to the sub-page "Festivals/Celebration").

As to the communal buffalo-sacrifices celebrated by some tribes of the Annamite Plateau, I append here the text from the Web page [http://www.batin.com.vn/dbotweb/uir/le\\_hoi/bs.htm](http://www.batin.com.vn/dbotweb/uir/le_hoi/bs.htm), having now been removed from the Web:

## BUFFALO SLAUGHTERING FESTIVALS OF THE ANNAMITE PLATEAU

Most ethnic groups in the High Plateaux hold the "Slaughtering buffalo" festival but with some differences in character and purpose.

One may detect four kinds of "slaughtering buffalo" festivals:

Those held after the harvest and at the beginning of a new agricultural season, and is therefore a harvest festival.

Those connected with the celebration of an important victory or with a sisters relationship between villages.

Those related to a ceremony designed to drive away evil spirits and protect a community (against epidemics or bad crop...) or related to the fate of a person (to cure illness or to express gratitude to God...).

Those related to a funeral or a marriage. These differences are also reflected in the scale and organization of the festival which may be:

Organized by family with the participation of the community where the

head at the family which sacrifices the buffalo as offering officiates at the ceremony.

Organized by a village, and so officiated by the village head. a combined ceremony between a family and the village, where the ceremony dedicated to the Earth God is officiated by the village head.

Organized jointly by several villages, for example, a ceremony Connected with a sisters relationship.

The venue of the festival is either in front of the "rung" house (communal house) or in the house of each family. We are giving below an account of the "Buffalo slaughtering" festival as organized by three major ethnic groups in the high Plateaux. Buffalo Slaughtering festival of the Jarai Ethnic Group The festival is held in front of the "rong" house. In the middle of the yard is planted the "Gingga", a wooden pole with carved images and decorated in various colors. Since early morning, a buffalo is tied to the "Gingga" while a big pig is also tied nearby. When all the villagers are present, the village head who officiates the ceremony approaches the "Gingga" and recites the prayers beseeching the Gods to accept the Festival offering and grant peace and prosperity to the village. The end of the prayers is greeted by yells and howls from the villagers and gongs and drum beats, which the hills and forests echo back. Then comes a feast of liquor which is drunk from a common jar with the help of bamboo tubes. This is followed by dances which boys and girls perform by joining hands with one another, to the accompaniment of the gongs and which last until late in the night. On the next day, drum and gong beats again resound, this time signaling the slaughter of the buffalo. This is preceded by a dance of young men armed with swords and shields, moving around in quick movements. Then comes the slaughtering of the buffalo. The young man who succeeds, with one stroke of his sword, in killing the buffalo is acclaimed as a brave. Then the buffalo is cut up and distributed in equal shares to members of the village. In particular, its head and horns are escorted to the "rong" house, while its blood is mixed up with liquor and used to create a valuable decorative item in the "rung" house such as weapons, gongs, horns, etc...

Buffalo Slaughtering Festival of the Bahnar Ethnic Group (dwelling mainly in Kontum province and in the mountainous areas of Binh Dinh, Phu Yen provinces). Several days prior to the festival, people set about planting the "Gung" pillar which, in fact, consists of 4 decorated and colored poles arranged as a place for the Gods to sit and witness the festival. Then a Kapok tree is planted in the yard for tying the buffalo. On the first day, the village head and 5 or 7 elders perform rituals in front of the "Gung" pillar. Thereafter, the villagers perform dances around the pillar to the accompaniment of gongs and cymbals.

A young man long lance, danced to the rhythm of drumbeats, of gong sounds, and screams of the crowd, and pierced the buffalo's heart. The buffalo remained standing and the lance was with drawn. At this moment a man standing to the side placed a bronze cauldron with some wine under the wound and caught the splashing blood. The village's oldest man went to the buffalo to cut some slices out of ears, nose and some hairs from its tail, which together with the blood were offered to the God. After the ritual the buffalo was carved and distributed to all the villages.

Buffalo-killing festivals take place in many parts of Tay Nguyen Highland. They may differ in time and orders of their organization, but the common point is that each ceremony is, in fact, a martial arts worship ritual, a festival where the young men of villages are able to show their skill in hunting. The second day is devoted to buffalo slaughtering. After a round of drum and gong beats, the young men set about killing the buffalo, with acclamation going to the person who succeeds in thrusting the sword into the heart of the animal. The buffalo is then cut up and given to the villagers in equal shares. In particular, its liver is cut into small pieces and distributed to the young men, because it is believed by the Bahnar people that buffalo liver will give tremendous strength to the person who eats it. Thereafter, villagers perform the "soang" dance, to the accompaniment of drums, gongs and cymbals, until late in the night. Buffalo Slaughtering Festival of The Ede Ethnic Group (dwelling mainly in Dac Lac province) A Kapok tree is also planted in the yard. When the festival begins, a buffalo is tied to that tree. The tree and the buffalo are usually surrounded by the crowd. After several rounds of drum and gong beats, the village head, who officiates the ceremony, offers prayers to the Gods and then opens the festival by striking the front leg of the buffalo with a knife. Then, a young man (designated in advance), armed with a knife, runs around the buffalo once and then strikes the hind leg of the animal with his knife. Seething with pain, the animal breaks free and runs away, but the young man runs after it and strikes the right hind leg. The animal falls down.

That's all for now.

Regards,  
Francesco Brighenti

| 22/2002-06-24 04:16:36|Juha Savolainen|Solheim on Madagascar|

Hi everybody!

My name is Juha Savolainen and I teach philosophy and

critical thinking in Helsinki. As one of my many interests, I am intrigued by the early history of South Asia and South East Asia and the role played by Austric/Austronesian speaking people in all this. I have a question (mainly targeted for Paul): Where can I find Wilhelm Solheim's comments on the archaeology of Madagascar?

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| 23|2002-06-28 08:32:58|pinatubo.geo|Re: Solheim on Madagascar|

Hi Juha,

I've had a hard time finding my notes on the relevant articles.

Try searching vol. 18 of \_\_Asian Perspectives\_ published in Honolulu. Also the 1986 issue(s) of the same journal. I'll get back to you with more precise references later.

Regards,  
Paul Kekai Manansala

--- In austric@y..., Juha Savolainen wrote:

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| 24|2002-06-28 21:37:35|TTT UUU|Re: Solheim on Madagascar|

I am interested in the Asiancolonization of Madigasar as well. The extent of Arabic (and other)trading in the Indian Ocean is not well known.Keep us posted.

**"pinatubo.geo"** wrote:

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Also the 1986 isssue(s) of the same journal. I'll get back to you with more precise references later.

Regards,  
Paul Kekai Manansala

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| 25|2002-06-29 08:49:41|a.manansala@attbi.com|Re: Solheim on Madagascar|

Here are a few refs. from off the web, although these might be a bit hard to get a hold of.

Solheim, W.G. 1967 From Southeast Asia to East Africa: An Archaeological Problem. CEAO Nairobi.

Solheim, W.G. 1965 Indonesian Culture and Malagasy Origins. Taloha I 33-42.

Regards,  
Paul Kekai Manansala

| 26|2002-06-29 13:41:52|juhav|Re: Solheim on Madagascar|

Paul, many thanks, although I will probably have hard time to find them here in Helsinki...Do you know whether there are any new excavations going in Madagascar to study the very earliest horizons?

What prompts me to ask this is partly the intriguing information you gave at your site about the spice trade: clearly, some of the exotic spices Egyptians and other Near Eastern people used could have come from Island South East Asia. So, I will now engage in frank speculation, just to identify areas that must be checked to figure out whether the speculation is on the right track or not.

The Speculation: Assuming that (perhaps very modest) trade with Island South East Asian spices was in existence already around 2000 BCE, who were the traders and what were their trade routes? My guess, perhaps unsurprisingly, would be that at least part of the trade was conducted by Austric speaking people living by the sea. And given our present knowledge of the population history of

Polynesia (the population of the Marianas etc.), it would not be unrealistic to assume long distance sea faring. Could they have found their way to the islands of Eastern Africa (Madagascar, Zanzibar etc.) already by 2000 BCE? Western Nusantara trading network?

Do we have any evidence suggesting either the correctness or the incorrectness of this speculation?

Regards, Juha Savolainen

--- In austric@y..., a.manansala@a... wrote:

>

> Here are a few refs. from off the web, although these might  
> be a bit hard to get a hold of.

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> Solheim, W.G. 1967 From Southeast Asia to East Africa: An  
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> Solheim, W.G. 1965 Indonesian Culture and Malagasy Origins. Taloha  
I 33-42.

>

> Regards,

> Paul Kekai Manansala

| 27|2002-06-30 13:23:39|an\_member|(no subject)|

In my dissertation on the ethnobotany of *Cordyline fruticosa*, which has been treated as sacred in different contexts in Southeast Asia, Papua New Guinea and Oceania, I began to think that it could be a substitute for a related plant that produced red resin called "dragon's blood." Now, I have written a paper on the subject, intended for



In a paper I have just written for the journal *Anthropos*, I argue against Robert Blust's thesis that the rainbow/dragon concept was reinvented because of natural human thinking about natural phenomena. He knows I disagree with him and he has been helpful before, but now I would like further advice about some of my thoughts and particularly about some of the names on my long list for *Cordyline fruticosa*. Any thoughts? It seems to me that this plant has been treated in the same way as "dragon's blood plants," *Dracaena* species, with which *Cordyline*s have been confused. My dissertation (1999) was on *Cordyline* ethnobotany and I have recently published on firewalking and *Cordyline* in the *Journal of the Polynesian Society* v. 110 (2). I would like help in interpreting a long list of names for both *Cordyline*s and *Dracaena*s. Can anyone help with this? I will be reading your postings with interest.

-----  
Dr. Celia Ehrlich  
254 Poverty Lane  
Lebanon, NH 03766-2702  
[cehrlich@cyberportal.net](mailto:cehrlich@cyberportal.net)

| 28|2002-06-30 19:22:26|a.manansala@attbi.com|Re: Solheim on Madagascar|

>

- > The Speculation: Assuming that (perhaps very modest) trade with
- > Island South East Asian spices was in existence already around 2000
- > BCE, who were the traders and what were there trade routes? My
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- > unrealistic to assume long distance sea faring. Could they have
- > found their way to the islands of Eastern Africa (Madagascar,
- > Zanzibar etc.) already by 2000 BCE? Western Nusantara trading
- > network?
- >

I think this is very possible. Technically, the ship-building ability was probably already there as ocean-going ships must have been used in the Nusantara trading network by at least 3,000 BCE.

The only question here is long-range navigation.

- > Do we have any evidence suggesting either the correctness or the
- > incorrectness of this speculation?
- >
- >

Depends on what type of evidence you are referring to. I've had people come up with theories based mostly on linguistics, then criticize me for not backing up my own views with hard archaeological artifacts.

The Ancient Egyptians claimed to have traded bulk commodities along with some weapons for the spices from the south.

So far, no real artifacts resembling the trade weapons shown on Egyptian reliefs have been found anywhere.

Some evidence of SE Asian spices have been found in ancient context in Africa and the Middle East. There were some cloves identified in 2nd millennium BCE jars in Syria. Tests of a Ptolemaic mummy also uncovered camphor from Borneo but this dated from only about 200 BCE.

The linguistic evidence regarding the Near Eastern words for cinnamon, cassia, etc. is tricky. However, from my own studies, I'm very confident that such an ancient trade existed.

Regards,  
Paul Kekai Manansala

| 29|2002-06-30 21:22:43|TTT UUU|Re: Solheim on Madagascar|

When I get a copy, this summer, of Stephen Oppenheimer's book Eden in the East it will probably address Indian Ocean trade and human dispersal. I recall that both Amazon.com and Barnes & Nobles sell it.

*juhavs* wrote:

Paul, many thanks, although I will probably have hard time to find them here in Helsinki....Do you know whether there are any new excavations going in Madagascar to study the very earliest horizons?

What prompts me to ask this is partly the intriguing information you gave at your site about the spice trade: clearly, some of the exotic spices Egyptians and other Near Eastern people used could have come from Island South East Asia. So, I will now engage in frank speculation, just to identify areas that must be checked to figure out whether the speculation is on the right track or not.

The Speculation: Assuming that (perhaps very modest) trade with Island South East Asian spices was in existence already around 2000 BCE, who were the traders and what were there trade routes? My guess, perhaps unsurprisingly, would be that at least part of the trade was conducted by Austric speaking people living by the sea. And given our present knowledge of the population history of Polynesia (the population of the Marianas etc.), it would not be unrealistic to assume long distance sea faring. Could they have found their way to the islands of Eastern Africa (Madagascar, Zanzibar etc.) already by 2000 BCE? Western Nusantara trading network?

Do we have any evidence suggesting either the correctness or the incorrectness of this speculation?

Regards, Juha Savolainen

--- In austri@y..., a.manansala@a... wrote:  
>  
> Here are a few refs. from off the web, although these  
might  
> be a bit hard to get a hold of.  
>  
> Solheim, W.G. 1967 From Southeast Asia to East Africa: An  
Archaeological Problem. CEAO Nairobi.  
> Solheim, W.G. 1965 Indonesian Culture and Malagasy  
Origins. Taloha  
I 33-42.  
>  
> Regards,  
> Paul Kekai Manansala

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| 30|2002-07-01 10:51:47|a.manansala@attbi.com|Re: (unknown)|

I argue

> against Robert Blust's thesis that the rainbow/dragon concept was  
> reinvented because of natural human thinking about natural  
> phenomena.

Dr. Ehrlich, could you give us some more specifics?

Also, would it be possible to post the list, or partial  
list of names, to the group?

Regards,  
Paul Kekai Manansala

| 31|2002-07-06 00:11:43|juhavs|Re: Solheim on Madagascar|

Hi TTT UUU...

Thanks for your alertness...however, I must confess here that I have read Oppenheimers book almost two years ago...It is very much worth reading...

Best regards, Juha Savolainen

--- In austrie@y..., TTT UUU wrote:

>

> When I get a copy, this summer, of Stephen Oppenheimer's book Eden in the East it will prob. address Indian Ocean trade and human dispersal. I recall that both Amazon.com and Barnes & Nobles sell it.

> juhavs wrote:

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> gave at your site about the spice trade: clearly, some of the exotic

> spices Egyptians and other Near Eastern people used could have come

> from Island South East Asia. So, I will now engage in frank

> speculation, just to identify areas that must be checked to figure out whether the speculation is on the right track or not.

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> The Speculation: Assuming that (perhaps very modest) trade with Island South East Asian spices was in existence already around 2000

> BCE, who were the traders and what were their trade routes? My

> guess, perhaps unsurprisingly, would be that at least part of the

> trade was conducted by Austric speaking people living by the sea.



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| 32|2002-07-06 00:17:53|juhavs|Re: Solheim on Madagascar|

Hi!

Paul, many thanks for your reply. Do you happen to know the precise source for the intriguing cloves discovery? - If one could make sure that the cloves was found from a stratigraphically secure context and that its dating could be corroborated, it could provide a very promising clue for further investigations.

Best regards, Juha Savolainen

--- In austrie@y..., a.manansala@a... wrote:

>

>>

>

>

>> The Speculation: Assuming that (perhaps very modest) trade with  
>> Island South East Asian spices was in existence already around  
2000

>> BCE, who were the traders and what were there trade routes? My  
>> guess, perhaps unsurprisingly, would be that at least part of  
the

>> trade was conducted by Austric speaking people living by the  
sea.

>> And given our present knowledge of the population history of  
>> Polynesia (the population of the Marianas etc.), it would not be  
>> unrealistic to assume long distance sea faring. Could they have  
>> found their way to the islands of Eastern Africa (Madagascar,  
>> Zanzibar etc.) already by 2000 BCE? Western Nusantara trading  
>> network?

>>

>

> I think this is very possible. Technically, the

> ship-building ability was probably already there as  
> ocean-going ships must have been used in the Nusantara  
> trading network by at least 3,000 BCE.  
>  
> The only question here is long-range navigation.  
>  
>  
>> Do we have any evidence suggesting either the correctness or the  
>> incorrectness of this speculation?  
>>  
>>  
>  
> Depends on what type of evidence you are referring to. I've  
> had people come up with theories based mostly on linguistics,  
> then criticize me for not backing up my own views with  
> hard archaeological artifacts.  
>  
> The Ancient Egyptians claimed to have traded bulk  
> commodities along with some weapons for the spices  
> from the south.  
>  
> So far, no real artifacts resembling the trade weapons  
> shown on Egyptian reliefs have been found anywhere.  
>  
> Some evidence of SE Asian spices have been found in  
> ancient context in Africa and the Middle East. There  
> were some cloves identified in 2nd millennium BCE jars  
> in Syria. Tests of a Ptolemaic mummy also uncovered  
> camphor from Borneo but this dated from only about  
> 200 BCE.  
>  
> The linguistic evidence regarding the Near Eastern words  
> for cinnamon, cassia, etc. is tricky. However, from  
> my own studies, I'm very confident that such an ancient  
> trade existed.  
>  
> Regards,  
> Paul Kekai Manansala

| 33|2002-07-09 12:51:53|pinatubo.geo|Re: Solheim on Madagascar|

--- In austriac@y..., "juhav" wrote:

>  
> Hi!  
>



> Paul, many thanks for your reply. Do you happen to know the precise  
> source for the intriguing cloves discovery? - If one could make sure  
> that the cloves was found from a stratigraphically secure context  
> and that its dating could be corroborated, it could provide a very  
> promising clue for further investigations.  
>  
> Best regards, Juha Savolainen  
>  
>  
>

TERQA AND HARADUM: A COMPARATIVE ANALYSIS OF OLD BABYLONIAN  
PERIOD  
HOUSES ALONG THE EUPHRATES  
<http://www.stlcc.cc.mo.us/fv/users/mfuller/aia/papers/chavalas/chavalas.html>

Lab tests have just been conducted on the samples. You can  
contact the lead researcher at:

[buccella@ucla.edu](mailto:buccella@ucla.edu)

Regards,  
Paul Kekai Manansala

| 34|2002-07-11 02:41:25|Juha Savolainen|Re: Solheim on Madagascar|

Paul,

many thanks for your reply!

Best regards,

Juha

--- "pinatubo.geo" <[a.manansala@attbi.com](mailto:a.manansala@attbi.com)> wrote:

> --- In austrie@y..., "juhav" wrote:

> >

> > Hi!

> >

> > Paul, many thanks for your reply. Do you happen to  
> know the precise

> > source for the intriguing cloves discovery? - If  
> one could make sure

> > that the cloves was found from a stratigraphically  
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> >

> > Best regards, Juha Savolainen

> >

> >

> >

>

>

>

> TERQA AND HARADUM: A COMPARATIVE ANALYSIS OF OLD  
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>

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>

> Regards,

> Paul Kekai Manansala

>

>

>

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| 35|2002-07-12 11:01:00|Oscar Nalesini|Madagascar, Moluccas and Mesopotamia|

As a new subscriber to the Austric newsletter, I received only the last 4 messages, but I found them interesting for they deal with a favorite topic of mine, i.e. ancient navigation in the Indian Ocean. This topic has had briefly touched recently in the congress "Ancient navigation and trade in the Indian Ocean", held in Ravenna, Italy, July 4-6.

The discovery of possible cloves in a II millennium BC Mesopotamian setting matches a few other archaeological evidences. These evidences I'm referring to are: a pendant discovered in a grave at Tell Asmar, and some goats' bones. The pendant is made of a resin presumably coming from Eastern Africa (between Zanzibar and Madagascar; Meyer, Todd, Beck, "From Zanzibar to Zagros: a copal pendant from Eshnunna", *Journal of Near Eastern Studies*, 50 (1991), p. 289-98), while the goats' bones comes from an excavation carried out by Ian Glover in Timor over 30 years ago, and have been recognised as of Indian stock (*Archaeology in Eastern Timor, 1966-67*, Canberra, Australian National University, 1986). These are the hard data, to my knowledge.

I do however support a different idea about who the traders were. Archaeological evidence of maritime trade in the Persian Gulf and in the Gulf of Oman is much more ancient. Recent British investigations in Kuwait discovered pieces of bitumen-based coatings of boats datable to ca. 5.000.

Earlier Italian and French excavations in Oman found about 300 pieces of such coating with the impressions of the reed-bundles and of the ropes binding them in the III millennium BC settlement of Ra's al-Junayz. There is now an ethno-archaeological experiment going on in Ravenna in collaboration with the Western Australia Maritime Museum aiming at the recovery of the ancient boat-building technology (see [www.maganboat.com](http://www.maganboat.com)). At least in the III millennium BC those were ocean-going ships, trading between Arabia and Northern India.

As you can see, there is no need to involve the presence of Austronesian or other eastern sailors in the Middle Eastern waters to explain the presence of such exotic goods in the II millennium BC Near Eastern sites, for the navigation capacity has had already attained at least a millennium before.

These evidences rather suggest that in the II millennium BC (and we don't know if for a while or for a longer period) the maritime trade circuits connected and allowed goods to travel from one edge of the ocean to the other: resin from Madagascar in Mesopotamia, and Indian goats in Timor. What is clear, however, is that the maritime people from both the western and the Eastern Indian Ocean mastered the technology of the Ocean navigation, and were able to contact each other. I don't think we will be able in the future to solve problems such as the origins of Malagasy language embracing hyper-diffusionistic ideas of Egyptians, Sumerians or Austronesians civilizing the entire world. We should rather look at the contacts of different people and the merging of their traditions into new cultures.

Oscar Nalesini

| 36|2002-07-12 11:34:05|a.manansala@attbi.com|Re: Madagascar, Moluccas and Mesopotamia|

I'm not sure what you mean by there "need to involve the presence of Austronesian or > other eastern sailors

in the Middle Eastern waters."

It is a theory as good or better than any other.

The actual historical evidence goes against an Indian connection as the spices come from Asia into Africa from a very obscure route.

Even well into the medieval period, Europeans did not acquire cinnamon and cassia from India, but instead from African ports. If you understand how these spices were traded in East Asia, the answers all become very obvious. Even there the trade was controlled by Yueh and Kun-lun maritime traders.

The obvious connection here are the Malagasy, whose route I think was almost certainly transoceanic.

Regards,  
Paul Kekai Manansala

> As a new subscriber to the Austric newsletter, I received only the last 4  
> messages, but I found them interesting for they deal with a favorite topic of  
> mine, i.e. ancient navigation in the Indian Ocean. This topic has had briefly  
> touched recently in the congress "Ancient navigation and trade in the Indian  
> Ocean", held in Ravenna, Italy, July 4-6.  
> The discovery of possible cloves in a II millennium BC Mesopotamian setting  
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> Zanzibar and Madagascar; Meyer, Todd, Beck, "From Zanzibar to Zagros: a copal  
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> are the hard data, to my knowledge.  
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> ethno-archaeological experiment going on in Ravenna in collaboration with the  
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> BC those were ocean-going ships, trading between Arabia and Northern India.  
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 > navigation capacity has already attained at least a millennium before.  
 > These evidences rather suggest that in the II millennium BC (and we don't know  
 > if for a while or for a longer period) the maritime trade circuits connected and  
 > allowed goods to travel from one edge of the ocean to the other: resin from  
 > Madagascar in Mesopotamia, and Indian goats in Timor. What is clear, however, is  
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 > origins of Malagasy language embracing hyper-diffusionistic ideas of Egyptians,  
 > Sumerians or Austronesians civilizing the entire world. We should rather look at  
 > the contacts of different people and the merging of their traditions into new > cultures.  
 >  
 > Oscar Nalesini  
 >

| 37|2002-07-12 13:05:10|Celia Ehrlich|Oscar Nalesini's pendant|

Oscar Nalesini mentions the discovery of a pendant from the second century BC discovered in a grave at Tell Asmar, and I wonder whether anyone has considered the possibility that it might be made of "dragon's blood" resin, which came from Dracaena trees in Eastern Africa as well as in Socotra. It was outgoing cargo on the route to India described in the Periplus of the Erythraean Sea (Balfour 1888). If the pendant were of "dragon's blood," a swab of dilute alcohol should remove a trace of color. Women in Socotra wore beads of the resin. Austric group members should know about "dragon's blood." It's an interesting story, well described in Schoff's translation of the Periplus.  
 Celia

-----  
 Celia Ehrlich  
 254 Poverty Lane  
 Lebanon, NH 03766-2702  
[cehrlich@cyberportal.net](mailto:cehrlich@cyberportal.net)

| 38|2002-07-13 03:32:15|Celia Ehrlich|Names for Socotran dragon's blood|

Isaac Bayley Balfour, who "discovered" the source of db mentioned by Dioscorides and Pliny, Socotran Dracaena

cinnabari, and reported it in the Transactions of the Royal Society of Edinburgh vols. 30 and 31, discussed the names given to the resin. The trees in Balfour's illustration are bristly and oddly branching. Burkill (1966: 871) repeats some of the names for the resin, but omits a key one: \_katir\_, the Arab name, saying that none of them applied to db or kino in SE Asia. Bent does mention \_katir\_ (1900: 379). Hirth and Rockhill (1911: 198n) also mention \_katir\_ and give Chinese characters for it, saying the name "may be the original form of \_hue kie\_, pronounced \_hut k`it\_ in Cantonese." Then, Hirth and Rockill fall into the same error McCrindle, apparently unaware of Balfour 1888, did in his translation of the Periplus; McCrindle (1973: 19-20) attributed db to "a leguminous tree." What was known, thanks to Humboldt, was db from *Dracaena draco* of the Canary Islands (taken there by Phoenicians?). *Dracaena* trees from Nubia also supplied db, but Malays began trading in substitute resins from rattans and "leguminous trees" of SE Asia and beat the competition, trade in db going the other way across the Indian Ocean.

Here is my question:

Could \_katir\_ (hut k`it) be the basis for Malay \_getah\_? Edward Schafer, in his article "Rosewood, Dragon's Blood and Lac" (1957: 133) saw a connection between Malay \_getah\_ and the Mandarin word he transcribes as "chieh." The resin Chau Ju-kuah mentions came from "the Zang countries," so could not have been the substitute resins.

'Sorry about the clumsy orthography.

Celia

-----  
Celia Ehrlich  
254 Poverty Lane  
Lebanon, NH 03766-2702  
[cehrlich@cyberportal.net](mailto:cehrlich@cyberportal.net)

| 39|2002-07-15 17:34:33|PlayaVerde@aol.com|South America|

Hello, I am looking for information supporting Polynesian ancestry in South American indigenous peoples. Does anybody know where I could find it? Genetic evidence would be best, but I would also appreciate archeological, linguistic, cultural, or any other. Thanks.

Regards, Eduardo

| 40|2002-07-16 10:44:35|Oscar Nalesini|Kunlun and dragon's blood|

Thank you very much for the stimulating critics to my message. As I don't find the answer to the question "who traded spices in the ancient Near East?" obvious, as claimed by Paul Manasala, I will add a few comments.

1) Paul Manasala mentions unspecified historical evidence against an indian connection in the maritime trade of the II millennium BC. I would really enjoy reading these historical sources, for I ignore them completely. So far as the sources I'm aware of are concerned, I don't see how he can deny the existence of such a trade. Mesopotamian clay tablets records the ships of Dilmun, Magan (Oman?) and Melluhha (Indus Valley) "tyed-up alongside the quay of Akkad" since the III millennium BC. There is a lot of discussion about what language the people of Magan and Melluhha spoke: elamite or semitic the former, dravidian the latter, maybe something else. One may think that Harappa people spoke an austric language (why not?), but still they were a people living in India. Incence and myrrh are south arabian and northeast african aromata.

In Roman times the presence of Indian traders in Egypt is documented by ostraca in tamil brahmi and pepper discovered at the Red Sea harbour of Berenike (and also at Qoseyr el-Qadim?). For Aksumite and Persian presence in VI century Sri Lanka, and Indian presence in Ethiopia, you can refer to the resume in Wolter's Early indonesian commerce.

It is always tempting to fill the holes of our ignorance projecting recent situations into the past, or generalizing local situations. Tempting means that they are good as working hypothesis, not as proof. The Kunlun are known from much later chinese sources (VII cent. AD, is it correct?), and are generally considered the coastal inhabitants of the Malay world. There is no question about their importance in the sea-borne trade of the South China Sea, exactly as there is no question of Malagasy as an austronesian language. But I don't see any proof that they were the only people able to navigate in the Indian Ocean, nor that they moored on the Near Eastern shores at least 2 millennia before.

2) Celia Ehrlich advances the hypothesis that the resin of the Tell Asmar pendant might be Dragon's blood (*Dracaena cinnabari*). It is of course a possibility, though I never heard of objects made with this vegetal resin: trade with Oman was normal in III millennium BC, and Socotra was inhabited at the same time. I never handled the object (I saw a picture), and I'm not even able to say how researchers carried out their lab analysis. I just read the results. As human beings, botanists make mistakes like me and you. On the other hand, one cannot doubt of their work just because he dislikes the results. I cannot say anything about the origins of the arabic name for the Dragon's blood. So far as I know this is an endemic tree of Socotra, so the word must be soqotri (south arabian branch of the semitic languages). The people who lived on the Indian Ocean exchanged ideas and words, besides goods. Considering the quantity of arabic and persian words in malay, some of them covering also meanings that had to be familiar in the Malay world well before the spreading of Islam (e.g. surat, nama, askar), it is hardly surprising that a malay world travelled the other way, but we need more data. The comparison with chinese words is always tricky: my pocket chinese dictionary (Beijing 1998) lists 59 characters under Jie and 16 under Qie (pinyin equivalents of Chieh). Which is the good one? Again, how did they pronounced it in Tang times? Don't overlook the chance of a simple resemblance between words conveying the same meaning: see for instance malay "dua" and italian "due". Still, all these evidences are chronologically much later.

| 41|2002-07-16 11:26:30|a.manansala@attbi.com|Re: Kunlun and dragon's blood|

- > Thank you very much for the stimulating critics to my message. As I don't find
- > the answer to the question "who traded spices in the ancient Near East?"
- > obvious, as claimed by Paul Manasala, I will add a few comments.

- > 1) Paul Manasala mentions unspecified historical evidence against an indian
- > connection in the maritime trade of the II millennium BC. I would really enjoy
- > reading these historical sources, for I ignore them completely.

I didn't say there was no trade between India and the Near East.

The historical sources I refer to are with regard to spices like cinnamon and cassia which came from African ports like Punt and Rhapta.

Even into Sassanian times, cinnamon and cassia were thought to come from Africa. The trade in these spices to the east was controlled by Austronesian mariners.

- >
- > Early indonesian commerce.
- > It is always tempting to fill the holes of our ignorance projecting recent
- > situations into the past, or generalizing local situations. Tempting means that
- > they are good as working hypothesis, not as proof. The Kunlun are known from
- > much later chinese sources (VII cent. AD, is it correct?), and are generally
- > considered the coastal inhabitants of the Malay world. There is no question
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- > there is no question of Malagasy as an austronesian language. But I don't see > any proof that
- > they were the only people able to navigate in the Indian Ocean,
- > nor that they moored on the Near Eastern shores at least 2 millennia before.
- >

No one said anything about mooring along the Near Eastern shores. You should read the posts more carefully. I'm referring to transoceanic trade between insular SE Asia and the east coast of Africa. From there the spices made their way north into Egypt and Arabia and from thence to Europe and elsewhere.

We know for a fact from Islamic sources that traders from Zabaj and Wak-wak in insular SE Asia were trading in East Africa during Muslim times. I'm simply suggesting that this trade existed much earlier times.



Regards,  
Paul Kekai Manansala

| 42|2002-07-19 10:02:04|Oscar Nalesini|R: [austriac] Kunlun|

Thanks again to Paul Manansala for his answer. So far as you suggest that a direct maritime trade between E. Africa and SE Asia might have existed earlier than the VIII-IX century, I agree with you: it was technically possible. I still disagree with you about the interpretation of the sources.

You quote cinnamon and cassia from Punt and Rhapta. You should read the sources more carefully. Rhapta is first mentioned in the *Periplus Maris Erythraei*. Chapter 17 says: "[...] the area [of Rhapta] exports a great amount of ivory [...], rhinoceros horn, best quality tortoise shell after the Indian (i.e. this is a local quality), a little nautilus shell" (Casson's translation). No cassia, no cinnamon, nor other spices.

The southernmost place where cassia was available in mid I century AD (again after the *Periplus*) was Opone, today Ras Hafun in Somaliland, and the other marts where cassia was sold are all between Ras Hafun and the Bab al-Mandeb. This fits with the informations of other Greek and Egyptian sources about the north-eastern african origins of both cassia and cinnamon.

On the other hand, if these spices were traded directly into E Africa by Austronesian sailors, how is it possible that these commodities were available only on the Gulf of Aden? And if the Austronesian sailors followed a northern route (across the Arabian sea to Yemen or the Horn of Africa) how can we explain that the same spices are not recorded as exports from the Golden Khersonese (only tortoise shells there) or Southern India (except indian spices as gangetic nard and malabathron)?

I don't find even evidence that the aromata bought in Punt by the Egyptians came ultimately from South-East Asia. I know the objection: cinnamon is the name of a south-east asian spice. Yes, this is how the Europeans called it in modern times. Names can move from one thing to another, especially if the latter substitutes the former. Do we have evidence that the Greeks and Roman "cinnamon" refers to the same botanic species the Middle Age Europeans bought in the Middle East, and later saw in the Indonesian archipelago?

In the Egyptian record we have the word out of which stemmed our "cassia". The biography of prince Herkhuf (lived around 2200-2100 BC) mentions a good called "s3t" from the land of j3m, along the Nile valley in modern Sudan. According to scholars of the Egyptian language, this word changed into "hs3t" a couple of centuries later, into "h3syt" in the late II millennium BC, and from this into the greek "kasia" and the hebrew "qsy'h" (sorry for the lack of diacritical marks!) not later than the VII century BC. We have evidence that "kasia" did not originate from the Nile valley, but from the desert and the mountains between the Nile and the Red Sea, i.e. Punt (Pwnt). We have no evidence that it was brought there from overseas, nor overland from more southern regions, though not all the Punt merchandises were necessarily local.

To make a long story short: I still don't find a way to support the idea that in Roman times (let alone the II millennium BC, the original topic of my first message) "The trade in these spices to the east was controlled by Austronesian mariners", (to quote your own words) nor that there was a "transoceanic trade between insular SE Asia and the east coast of Africa", from where "the spices made their way north into Egypt and Arabia and from thence to Europe and elsewhere". As I said above, it was technically possible for the Austronesian sailors to trade with Eastern Africa, and probably they did (how else can we explain Madagascar?), but there is no evidence of their presence in the historical records for the "Roman" or earlier times, and above all there is no evidence that the most famous spices of the Erythraean Sea commerce were Southeast Asia's. If we want to discuss such topics, we must look for firm ground, not suppositions.

Regards

Oscar Nalesini

| 43|2002-07-20 06:57:32|a.manansala@attbi.com|Re: R: [austric] Kunlun|

- > Thanks again to Paul Manasala for his answer. So far as you suggest that a
- > direct maritime trade between E. Africa and SE Asia might have existed earlier
- > than the VIII-IX century, I agree with you: it was technically possible. I still
- > disagree with you about the interpretation of the sources.
- > You quote cinnamon and cassia from Punt and Rhapta. You should read the
- sources
- > more carefully. Rhapta is first mentioned in the Periplus Maris Erythraei.
- > Chapter 17 says: "[...] the area [of Rhapta] exports a great amount of ivory
- > [...], rhinoceros horn, best quality tortoise shell after the Indian (i.e. this
- > is a local quality), a little nautilus shell" (Casson's translation). No cassia,
- > no cinnamon, nor other spices.

You definitely need to do more reading on this subject.  
Periplus may or may not be the oldest source depending  
on which dating you use.

Read also Pliny the Elder, Theophrastus, Strabo, Pomponius  
Mela, Ptolemy and many other authors all the way to the  
time of Cosmas Indicopleustes.

They mention the spice trade including cassia and cinnamon  
either in Rhapta or in the area around or south of  
Azania known as Regio Cinnamomifera.

After you get acquainted more with this subject, we can  
move to discussion of Punt.

Regards,  
Paul Kekai Manansala

| 44|2002-07-20 06:59:59|a.manansala@attbi.com|Fwd: The Spice Trade|

I like to forward an article of mine from one of this  
group's websites.

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The Spice Trade

The spice trade played a major role in during the European colonial period. The lure of spices and precious metals drove Europe to explore new trade routes to the "East Indies" (Malay Archipelago).

The spice trade, though, actually began much earlier than this period. The sweet-smelling clove which was so valued during in colonial Europe was known much earlier to the ancient Chinese and Indians. This, despite the fact that before the modern period cloves grew only in the Moluccas Islands of Indonesia.

In ancient China it was required that people first chew cloves (gui) before speaking with the Emperor to ensure a pleasing breath. The Indians knew of cloves (lavanga) in their Ayurvedic system of medicine and used them to fasten betel leaves around areca nuts when chewing the betel quid.

But cloves have been found as early as 1,700-1,600 BCE in Terqa, Syria where a pot of cloves was found by archaeologist G. Buccelatoi . This date is very interesting because not too far away in Egypt during the reign of Queen Hatshepsut (BCE 1503 to 1482), there is mention in the hieroglyphic texts of ti-sps 'cinnamon wood' being imported from the southern land of Punt.

Now cinnamon also grew only in southeastern Asia before the modern period. Cinnamon is again mentioned in the 7th century BCE Hebrew biblical texts and by Theophrastus (B.C.E.372-288).

The Hebrews claimed that cinnamon, cassia and possibly also lemon-grass were used in the holy oil of the temple proscribed by Moses (Exodus 30). This correlates somewhat to the early dates for cloves in Syria and cinnamon in Egypt.

The Hebrew word for cinnamon 'quinamom' is believed to be derived from Malay kayu manis 'sweet wood,' and the English word is ultimately derived from the Hebrew.

How did these spices arrive in Africa and the Middle East?

Archaeologists have found that the medieval cinnamon trade followed a route across the vast expanse of the Indian Ocean from Indonesia to Madagascar. The island of Madagascar has been occupied by Austronesian people since at least the start of this era. However, the late archaeologist Wilhelm Solheim thought their arrival may have been much earlier.

Solheim wondered why archaeologists in Madagascar started from the most recent finds back in time rather than the usual method of attempting to find the oldest period and working forward in time.

He had found much unstratified evidence suggesting there could have been a much earlier presence of Austronesians in this area. For example, types of Austronesian pottery dating well before the accepted arrival date have been found, but not in the pristine layers of sediment required. For example, sherds of such pottery may be found washed up on sandy beaches. Obviously the simplest explanation would be that the pottery had been unearthed locally in a flood and washed into the ocean by rivers.

Whatever the date of the Austronesian arrival, in medieval times, cinnamon landing in Madagascar would then find its way to Rhapta in present-day Somalia. From there it reached the South Arabian traders in Muza, Yemen who carried on the trade along the coasts of the Red Sea.

Now, this could be a medieval model explaining the ancient ships of Hatshepsut which sailed to the southern country of Punt to obtain cinnamon amongst other goods!

In the 3rd century ACE, the Chinese historians wrote about the sea-going vessels of the Kunlun of insular Southeast Asia. The Kunlun were described as short, wooly-haired, dark-skinned people who were expert sailors.

The Kunlun vessels were known as kunlun-po with the word po being the Kunlun word for ship. This could be cognate with Austronesian proa from Proto-Austronesian \*peDaHu/\*paDaHu.

Pliny the Elder (23/24-79 C.E.) wrote of how the Ethiopians bought cinnamon from their neighbors (probably to the south) who purchased it themselves from a seafaring people. These mariners:

"bring it over vast seas on rafts which have no rudders to steer them or oars to push or pull them or sails or other aids to navigation; but instead only the spirit of man and human courage. What is more, they put out to sea in winter, around the time of the winter solstice, when the east winds are blowing their hardest. These winds drive them on a straight course, and from gulf to gulf. Now cinnamon is the chief object of their journey, and they say that these merchant-sailors take almost five years before they return, and that many perish. In exchange they carry back with them glassware and bronze ware, clothing, brooches, armlets, and necklaces. And that trade depends chiefly on women's fidelity to fashion."

The ships of the Kunlun described by Chinese historian Wan Chen were much more impressive sometimes extending to 200 feet in length and standing out of the water up to 20 feet. They were said to be able to hold from 600-700 passengers and 10,000 bushels (900 tons) of cargo. Each ship could sport up to four obliquely mounted sails.

The Periplus of the Erythraean Sea, written in the first century ACE, describes ships from Chryse in the Malay Archipelago known as Kolan-diphonta or 'Kolan-ships'. The 'Kolan' here are probably the Kunlun of the Chinese annals.

The ships are described as large vessels made of two whole logs roped together. This sounds very much like the double canoes of Oceania. Stone representations of such canoes have been found in Indonesia and northeastern India. Waruno Mahdi thinks these were the original models from which we get Proto-Austronesian \*peDaHu/\*paDaHu.

The trading voyages to Africa continued well in the Muslim period. The 12th century Arab geographer El Idrisi writes in Kitab Rujar about the inhabitants of

Zabaj, a general term for Insular Southeast Asia who travel to Sofala and Zanj on the coast of sub-Saharan Africa:

"The residents of the Zabaj go to the land of Sofala and export the iron from there supplying it to all the lands of India. No iron is comparable to theirs in quality and sharpness."

"The inhabitants of Zabaj call at Zanj in both large and small ships and trade their merchandise with them, as they understand each other's language."

When the Europeans arrived in the area at the start of the Spice Trade, there were still references to trade missions from the Malay Archipelago as far as India and the Maldives, but no mention is made of trade with Africa.

The link with Madagascar and East Africa had apparently severed. The reasons are not altogether clear. The Europeans would soon learn just where all those spices were coming from and a new chapter in the spice trade would begin.

However, for thousands of years it appears that it was the navigators and sailors of Austronesia who controlled the spice.

| 45|2002-07-20 08:39:18|a.manansala@attbi.com|Some standard reading on spice trade|

A little dated but still an important work on the spice trade:

Miller, James Innes. \_ The spice trade of the Roman Empire\_. Oxford, Clarendon P., 1969.

Also, more recent:

Brierley, Joanna Hall. \_Spices : the story of Indonesia's spice trade\_. Kuala Lumpur ; New York : Oxford University Press, 1994.

Regards,  
Paul Kekai Manansala

| 46|2002-07-21 10:29:10|Oscar Nalesini|R: R: Kunlun|

Dear Mr Manasala,

you wrote:

- 1) You definitely need to do more reading on this subject.
- 2) Read also Pliny the Elder, Theophrastus, Strabo, Pomponius Mela, Ptolemy and many other authors all the way to the time of Cosmas Indicopleustes. They mention the spice trade including cassia and cinnamon either in Rhapta or in the area around or south of Azania known as Regio Cinnamomifera.
- 3) After you get acquainted more with this subject, we can move to discussion of Punt.

There are two ways to deal with others' different ideas: you can produce the evidence of their errors, or avoid to answer the other's questions diminishing him as ignorant. You apparently chose the second way, while I continue in the first.

The Periplus is the oldest source mentioning Rhapta, if you agree on a 1 cent. AD dating as stated in my message (and as in your forwarded article; in your last response, however, you held a different opinion: "Periplus may or may not be the oldest source depending on which dating you use").

But let's go back to the main point. You will have a hard time looking for Rhapta in Pliny the Elder, Theophrastus, Strabo and Pomponius Mela. You will find Azania instead (not in Theophrastus, to my memory), but Azania starts in the Horn of Africa, while Rhapta was far to the south.

I know you have a different opinion even on this point. In your forwarded article you say that Rhapta was in present Somalia. This is hardly possible, for the distances given by the Periplus are quite longer: from Opone to Rhapta 25 dromoi (dromos is the distance sailed in one day. It varied from 1,000 to 4-500 stades, depending on wind and sea conditions, Ptolemy, Geogr. hyph. I 9 and 17). The same distance is in Diogenes's records, while Theophilus has 20. The current hypotheses locate Rhapta between Dar es-Salam and the delta of the Rufiji.

Pliny (n.h. IX 86) doesn't say that "Ethiopians bought cinnamon from their neighbors who purchased it themselves from a seafaring people". This translation is wrong. What Pliny really says is that cinnamon grows in that part of Aethiopia where the people intermarry with the Trogodytes ("Omnia falsa, si quidem cinnamomum item cinnamum nascitur in Aethiopia Trogodytis conubio permixta"). The Trogodytes were the inhabitants of the Red Sea littoral (Trogodytica). As there is no mention of a third people involved in this trade, we can only conclude that they were the mariners who "bring it over vast seas on rafts which have no rudders to steer them or oars to push or pull them or sails or other aids to navigation; but instead only the spirit of man and human courage".

But don't worry. I'm not going to suggest that African mariners went to SE Asia to buy spices. It should be quite obvious that you cannot ply the waves of the Indian Ocean on rafts without rudder, oars and sails! You cannot even take the five years as a proof for the transoceanic journey: if you have the technical skill to cross the Ocean, and then you have to wait the monsoon to come back, this is really too long. Their range was certainly more limited. As a matter of fact, Pliny describes their route in the following paragraph (88; please, next time check the original text! you omitted an important sentence): "Hi recto cursu per sinus impellunt, atque a promontorii ambitu Argetae deferunt in portum Gebbanitarum qui vocatur Ocilia.

Quamobrem illi maxime id petunt, produntque uix quinto anno reuerti negotiatores et multos interire". It should be clear that they called for the South Arabian harbour of Ocilia/Okelis, and that someone remained there (in Ocilia/Okelis) five years, for that was their favourite place (illi maxime id petunt). On the location of cinnamon according to Pliny see also ch. XII 42.

The only text where Kinnamomophoros (Regio cinamomifera) lies south of the Horn of Africa is Ptolemy's Geography, but in the interior of Africa, not on the coast! In earlier references, from Theophrastus and Eratosthenes, the position is definitely northern. Theophrastus has the cinnamon from Arabia (h.p. IX 4,2 and 7,2), as Mela later (III 68, or 71: Arabia dicitur, cognomen Eudamon, angusta, verum cinnami et turis aliorumque odorum maxime ferax). According to Strabo II 5,35, Regio cinamomifera corresponds to the region where they hunted elephants (h<sup>1</sup>innam<sup>1</sup>h<sup>1</sup> estin, eph' h<sup>1</sup>고h<sup>1</sup>laph<sup>1</sup>駝ne th<sup>1</sup>경 t<sup>1</sup>lai<sup>1</sup> and we know from both literature and inscription that these hunting places were on the Red Sea, the southernmost probably being near Adulis (Monumentum adulitanum I, after Cosmas).

The evidence of the Periplus is significant in this respect: as I said in my former post, the sole detailed document on the Indian Ocean trade of Roman times we have, clearly says that cassia was available only in the marts of the Gulf of Aden. Moreover, there is no mention of cassia or cinnamon as exports from south Indian or SE Asian (Golden Khersonese) harbours. I repeat my (still unanswered) questions. If these spices were traded directly into E Africa by Austronesian sailors, how is it possible that these commodities were available only on the Gulf of Aden? And if the Austronesian sailors followed a northern route (across the Arabian sea to Yemen or the Horn of Africa) how can we explain that the same spices are not recorded as exports from the Golden Khersonese (only tortoise shells there) or Southern India (except Indian spices as gangetic nard and malabathron)?

In the Greek and Latin geographical literature we mainly have two "parties" about the origins of cinnamon and cassia: the first follows Herodotus' tradition, claiming they came from Arabia, the second from the Horn of Africa. This difference probably reflects shifts of the commercial routes on which these merchandises travelled: sometimes through the Arabian caravans, and sometimes from the Nile or the Red Sea.

There is a third "party", supporting an Indian origin of cassia, precisely in Mousicanus' kingdom (Strabo, XVI 4,25). But this is a tradition, I think, based on Aristobulos' descriptions of the aromatics found by the army of Alexander in north-western India.

This is, in summary, what we can get from the Greek and Latin sources about the trade of cinnamon and cassia. My conclusion still is that there is no evidence in these sources for the transoceanic trade which, in your opinion, brought cassia and cinnamon from SE Asia into E Africa in Hellenistic-Roman times.

Regards

Oscar Nalesini

| 47|2002-07-21 13:17:58|a.manansala@attbi.com|Re: R: R: Kunlun|

- > The only text where Kinnamomophoros (Regio cinamomifera) lies south of the Horn
- > of Africa is Ptolemy's Geography, but in the interior

of Africa, not on the > coast! In earlier references, from Theophrastus and Eratosthenes, the position



> is definitely northern. Theophrastus has the cinnamon from Arabia (h.p. IX 4,2  
> and 7,2), as Mela later (III 68, or 71: Arabia dicitur, cognomen Eudamon,  
> angusta, verum cinnami et turis aliorumque odorum maxime ferax). According to  
> Strabo II 5,35, Regio cinamomifera corresponds to the region where they hunted  
> elephants (h? Kinnam?moph?ros estin, eph' h?s h? t?n elaph?nt?n g?gone th?ra t?  
> palai?n), and we know from both literature and inscriptions that these hunting  
> places were on the Red Sea, the southernmost probably being near Adulis  
> (Monumentum adulitanum I, after Cosmas).  
>

Actually the Regio cinnamomifera, which you did not even bother to mention, despite your claims of evidence-based arguments is widely placed on the SE coast of Africa by modern scholars based on works like Strabo, Cosmas, etc. They clearly place the location of the cinnamon market in or south of Azania, not in Arabia.

Here are some maps of the Regio based on Strabo and Eratosthenes:

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Msg #  
From: "pinatubo.geo"  
Date: Wed Feb 13, 2002 10:57 am  
Subject: Cinnamomifera regio in the maps of Strabo and Eratosthenes

#### ADVERTISEMENT

<[http://us.a1.yimg.com/us.yimg.com/a/1-/flash/ediets/rememberwhen\\_wedding\\_070802.jpg](http://us.a1.yimg.com/us.yimg.com/a/1-/flash/ediets/rememberwhen_wedding_070802.jpg)>

Here are two links to the maps developed from Strabo (18 AC) and Eratosthenes (194 BCE) that show that both thought the cinnamon-producing country (Cinnamomifera regio) was in the extreme southeast of Libya (Africa).

Eratosthenes map  
<http://www.henry-davis.com/MAPS/Ancient%20Web%20Pages/112.html>

Strabo map

<http://www.henry-davis.com/MAPS/Ancient%20Web%20Pages/115.html>

> The evidence of the Periplus is significant in this respect: as I said in my  
> former post, the sole detailed document on the Indian Ocean trade of Roman times  
> we have, clearly says that cassia was available only in the marts of the Gulf of  
> Aden. Moreover, there is no mention of cassia or cinnamon as exports from south > Indian or  
> SE Asian (Golden Khersonese) harbours. I repeat my (still unanswered)  
> questions. If these spices were traded directly into E Africa by Austronesian  
> sailors, how is it possible that these commodities were available only on the  
> Gulf of Aden? And if the Austronesian sailors followed a northern route (across  
> the Arabian sea to Yemen or the Horn of Africa) how can we explain that the same  
> spices are not recorded as exports from the Golden Khersonese (only tortoise  
> shells there) or Southern India (except indian spices as gangetic nard and  
> malabathron)?

Because they may not have come from the Golden Khersonese  
(Malaysian Peninsula).

I should note that cinnamon (not malabathron) and cassia  
are not recorded as products of India either in ancient  
or medieval works up to the time of Cosmas.

The strongest evidence in Greek and Latin texts is that  
cinnamon and cassia come from the SE coast of Africa.  
Again, I suggest you do more reading rather than  
giving a slanted and selective overview of the evidence.

Even into colonial times, the primary sources of both  
cinnamon and cassia were in SE Asia.

Regards,  
Paul Kekai Manansala

| 48|2002-07-21 13:43:47|a.manansala@attbi.com|Re: R: R: Kunlun|

And if the Austronesian sailors followed a northern route (across

> the Arabian sea to Yemen or the Horn of Africa) how can we explain that the same  
> spices are not recorded as exports from the Golden Khersonese (only tortoise  
> shells there) or Southern India (except indian spices as gangetic nard and  
> malabathron)?  
>

I want to revisit the problems with this argument.

If the Greek and Roman sources had some limited knowledge of trade from SE Asia going west, how much more so would they know about trade from India to Arabia?

Yet, the ancient sources don't mention cinnamon as a product of India. Also, cassia is not mentioned as a product of the Silk Road.

Why? Cassia trade in Asia was in the hands of Yueh traders. The evidence also points to Indonesian cinnamon and not the Ceylon variety as the used in the ancient trade. Even the Sassanians were unfamiliar with Ceylon cinnamon. The early Arabs knew of cinnamon as a product from east of India, before the discovery of Ceylon cinnamon around the time of Ibn Batuta.

The reason the Greco-Roman sources did thought the spices came from Africa is because they came to East African ports from Asian sources that were obscure to them.

Even the other products of Rhapta, coconuts, aloe wood, tortoise shell, etc. are known to have come from SE Asia. It doesn't take much logic to follow the trail, when one knows from where the Malagasy, Wak-wak, Zabaj, etc, originated.

Regards,  
Paul Kekai Manansala

| 49|2002-07-23 10:38:34|TTT UUU|Re: South America|

The sweet potato made its way across the Pacific from S. America so either the S. Americans took it west or Polynesians (or others) arrived and brought it back west with them. The dispersal of the sweet potato can give you clues of human movements.

R. Day

**PlayaVerde@aol.com** wrote:

Hello, I am looking for information supporting Polynesian ancestry in South American indigenous peoples. Does anybody know where I could find it? Genetic evidence would be best, but I would also appreciate archeological,

linguistic, cultural, or any other. Thanks.

Regards, Eduardo

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| 50|2002-07-23 18:13:00|pinatubo.geo|Fwd: Did globalization touch ancient civilizations? |

An interesting editorial, not meant to provoke anybody,  
just interesting reading.

<http://www.asahi.com/english/tenjin/K2002072300418.html>

Did globalization touch ancient civilizations?

The Asahi Shimbun

In a land that is home to an ancient civilization, you may be struck by a sense of familiarity. You may get the same feeling even at a museum exhibition.

It may be deceptive. But you still cannot help feeling that despite the great distance between Japan and the faraway land, our ancient ancestors somehow had links with the people who lived there.

To give some examples, I feel this way about the Maya and Aztec civilizations that flourished in Central America. My sense of familiarity is evoked specifically by images of the sacred beast Quetzalcoatl (a snake with wings). They remind one of the lion head used in Japan for the lion dance (shishimai) that is performed in traditional celebrations. They look exactly like the Barong, the lion likeness of Bali, Indonesia.

In fact, Indonesian civilization influenced the civilizations of Central America, according to Eiji Hattori, professor at Reitaku University. The other day, the professor made public this view in a

speech he delivered under the title of ``Did the dragon cross the Pacific?" at a meeting of the International Society for the Comparative Study of Civilization.

Hattori long worked for UNESCO headquarters, and has visited the ruins of ancient civilizations throughout the world. This experience lends conviction to his argument.

In his speech, the professor pointed out that artifacts from Indonesia and Central America attest to a commonly held view that snakes gave birth to human beings, that this view provided a basis for icon production, and that similar pyramid-like architectural styles were followed. Considering these similarities, he said it was highly likely that the two civilizations across the Pacific had come in contact.

He added that ocean currents and ancient navigational techniques also made it possible.

In an interview, I suggested to Hattori that the similarities might have been accidental.

``People anywhere entertain similar ideas, you know," I said. ``So don't you think it may be possible to consider the similarities you mention as an accidental outcome of efforts pursued separately to create a civilization?"

In response, the professor said: ``We are dealing with peculiar perceptions, peculiar icons and peculiar architectural styles. It would make more sense to think that they resulted from contact with another civilization."

He added: ``There was no need to lay hands on things. Information was all that they needed to have."

In the age of globalization, information gets around the world in a flash. It occurred to me that taking time out in an era of hectic activity for the leisurely pursuit of musing on the ``sea road" that linked ancient civilizations was not such a bad idea.

--The Asahi Shimbun, July 22

| 51|2002-07-24 01:36:34|Oscar Nalesini|R: [austrie] R: R: Kunlun|

Dear Mr Manasala,

The maps you consulted in the web do not "show that both [Eratosthenes and Strabo] thought the cinnamon-producing country (Cinnamomifera regio) was in the extreme southeast of Libya". They are

replicas of the old (1879) Bunbury's maps. They are still good to learn the general shape of the world according to the ancient authors, but to extrapolate a precise location of a region from maps on that scale is really a fancy idea.

As though this was not sufficient, you choosed the wrong authority. I suggest you to stop looking for casual finds on the web, and rather go into a good library, where you will almost certainly find a copy of Bunbury's handbook (let's take Strabo too) and will learn how he constructed those maps and located the Regio cinnamomifera on them: you will discover that also in Bunbury's opinion Strabo meant in or very close to the Horn of Africa, and in the interior, not on the coast (where also Ptolemy located it over 100 years later). You may suppose that the Cinnamon country, as conceived by the Greek authors, was a trading place rather than the country where Cinnamon actually grew (and you never said this), but even this is a supposition, not a fact.

The geographical knowledge of the eastern Africa's coast in the late II century BC, and still good in the next, among the people of the early Roman empire is reported by Strabo himself: "After rounding the promontory [of Notu Keras, i.e. Cape Guardafui] approximately towards the south, we no longer, he [Agatharchides of Cnidus] says, have any record of harbours or places, because the promontory [of Notu Keras] is not known from here on, and the same is true of the coast next to it" (XVI 4,14).

This for the geographical aspect of your last comments. But there is also a (major) methodological aspect: Bunbury's map is an interpretations of Strabo's texts, exactly like mine and yours. You can agree or disagree with an interpretation, but you cannot take it for the proof of what Strabo really said! The difference between primary and secondary sources is basic to historical research; the correct way to demonstrate that I misunderstood Strabo is not opposing others' opinions, but using Strabo himself. This is what you did not do so far.

Some other points struck me.

1) I quoted (and gave precise indications, sometimes also the original text) of primary Greek and Latin sources concerning the trade in cinnamon and cassia, as well as the location of countries and places, but you never replied showing where and why my reading of the sources is wrong. On the contrary, you reply only with bold assertions. "The strongest evidence in Greek and Latin texts is that cinnamon and cassia come from the SE coast of Africa" is one of them, for you don't show where the Greek and Latin sources say this.

2) How can you say that I "did not even bother to mention" Regio cinnamomifera after quoting many lines of mine discussing exactly that topic?

3) If I gave only a "slanted and selective overview of the evidence", why don't you list the missing passages?

4) Is it for lack of attention that you never give a precise reference to any author, nor discuss the texts, but only dispense names and judgements?

My answer to these questions is: you can't support your opinions with the primary sources because you are quite unfamiliar with Greek and Latin literature and with textual criticism, and therefore you can only repeat the opinion of some other scholars (Innes Miller seems the favourite).

Now, let's turn the attention to the second e-mail, where you wrote:

1) "yet, the ancient sources don't mention cinnamon as a product of India. Also, Cassia is not mentioned as a product of the Silk Road."

I don't understand your mention of the Silk Road. Nobody so far mentioned it. What I said, is that there were ancient authors believing that cassia came from India, and quoted as an example of this opinion a passage of Strabo concerning the Musicanus' kingdom (Strabo's sources said this, not me, and I also

gave a possible explanation of it). In the case your reference to the Silk Road is a response to my short note on the Indian cassia, can you explain me what has Musicanus to do with the Silk Road?

After this (unlucky) sentence I read:

2) "Cassia trade in Asia (sic!! does it include Siberia, Japan, Tibet and Caucasus?) was in the hands of Yueh traders".

So far as I know, there is no textual evidence about Yueh traders beyond the South China Sea in the time of the Greek sources we are discussing. In the case they exist, I would like to know them.

3) "The evidence also points to indonesian cinnamon and not the Ceylon variety as the used in the ancient trade"

Once again your evidence is mystic rather than textual or archaeological. I don't know of any sample of cinnamon found in western Indian Ocean archaeological sites. If someone made such a discovery, there should be a bibliography. Otherwise I would like to know how you identified the varieties.

4) "Even the other products of Rhapta, coconuts, aloe wood, tortoise shell, etc. are known to have come from SE Asia."

You definitely don't read the sources: you invent them. No text mentions exports of coconuts and aloe from Rhapta in Roman times (if I missed something, please...). There was tortoise shell in Rhapta: "the best quality after the Indian" according to the Periplus. The same source says that the tortoise from Khryse (Golden Khersonese) was "the finest of all the places on the Erythraean Sea", so it follows that the tortoise sold in Rhapta was a different one. Indonesian? Hardly possible. All the merchandises from Rhapta were available in Africa (ivory, rhinoceron's horn, tortoise, sea-shells), and there is no way to suppose on our meagre written evidence that they were imported from SE Asia. The evidence of a later period (Sasanians and muslim Arabs) is not a proof that the same happened centuries before. Today Canada sells wheat to Russia, but this does not proof that the Athapaskans sold wheat to the Sarmatians.

It is perfectly fine if you try to fill a gap with an hypothesis, but you have to substantiate it with proofs, or at least sound arguments, before going on.

You cannot assume that the sources say what they don't say (the Romans received cassia and cinnamon from SE Africa), then advance an hypothesis on this base without a single proof (they were SE Asian spices arrived through transoceanic trade), and finally take your hypothesis for an established historical fact and build a new supposition over it (the trade was in the hands of Yueh and Kunlun traders). The result of this method is fiction, not history.

Regards

Oscar Nalesini

| 52|2002-07-24 05:58:09|a.manansala@attbi.com|Re: R: [austrie] R: R: Kunlun|

Again Mr. Nalesini, you seem to have taken time to go to the library to fill yourself in on details that you apparently were unfamiliar with before.

According to your own type of argument, there is also not a "single

proof" that cinnamon and cassia trade came via India to Arabia as you suggest. Despite the fact that this would be much easier to ascertain than a southern route.

The fact that you don't know about Yueh traders in "Greek times" again shows that you want me to provide references that you are unaware of.

Seafaring experts like Hornell and Haddon had long noted that outrigger ships like the mtepe of East Africa were probably derived from Austronesian ships. These may have received first mention in the Periplus which talks of boats sewn with coconut fiber (as with the mtepe).

Of course, the coconut of East Africa is also thought to have been brought by Austronesians. Aloe wood was a direct product of SE Asia, and in Muslim times tortoise shell was connected directly with SE Asia and it is even mentioned as a product traded from there in African markets.

You mention Herodotus obscure' references to cinnamon as traded by the Arabians but even that early author indicates that the exact source of Arabian cinnamon was unknown to him. "[3.111] Still more wonderful is the mode in which they collect the cinnamon. Where the wood grows, and what country produces it, they cannot tell - only some, following probability, relate that it comes from the country in which Bacchus was brought up."

In latter times, it is clear that Greek and Roman authors, as they became more familiar with southeastern Africa, were aware that cinnamon and cassia came into Arabian markets from Africa. Interesting how you try to minimize Ptolemy whose geographical knowledge was later and obviously superior to the earlier writers.

Ptolemy clearly locates major spice markets on the coast of Africa as did later writers including Cosmas. Of course, they could have only vague ideas where these Asian spices were coming from since they followed a southern oceanic route. If they had come from Indian and Arabia, this should have been very clear, but the writers instead thought spices in Arabian markets came to the Arabians from Africa!



And, of course, they never place the *Regio cinnamomifera* in Arabia or India. So where is your own "single proof?"

Now if you do some more preliminary research you will find that cassia and cinnamon were considered products of areas east of modern India well into Muslim times. Cinnamon harvesting in Sri Lanka begins only after the importation of the Tamil-speaking harvesting caste in the 13th century.

Well before this time southern traders were dealing in spices like cassia, cinnamon and cloves with China. From the Chinese annals themselves it is clear that they had no idea of cinnamon coming from India in early times. Both cinnamon and cassia are products of the countries of the Nanyue (before and after this region's conquest) or further south.

I will wait for your answer after your next library trip.

Regards,  
Paul Kekai Manansala

| 53|2002-07-24 08:18:31|travelershome|origins of the sumerians|

i am looking for the starting place of the sumerians or rather what they called themselves the sag-giga. so far this is what i have foudn. they came from the northern marshes of the caspian sea, from meotis. after they arrived in the tigris area, by their neighbors they lived in kengir-the land of many river. they called their language emegir. i have found a conflicting story about their language and what group it belongs with. one is austric, the other ural altai. i have also found connections with ancient turanains origins the sumerian-scythian-hun-magyar identity and continuity . current language connected best is hungarian 51 of 53 grammar points connect. [www.igs.net](http://www.igs.net) for that last bit. so here is my basic question- where did they come from, how did they seems to appear on the scene with such an advanced approach to existence. i have read that their religion says they came from another planet. i have read that the preceeding elamite culture helped them develop theirs, and i have read that clearing the marshes and controlling the waters in that area sped them on their

way towards what is now known as the most advanced culture in the ancient world. can anyone cnnected with this site assist me? pamela pallessen-scherehart

=====

---

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| 54|2002-07-25 07:52:35|pinatubo.geo|Re: South America|

--- In austrie@y..., TTT UUU wrote:

>

> The sweet potato made its way across the Pacific from S. America so

either the S. Americans took it west or Polynesians (or others) arrived and brought it back west with them. The dispersal of the sweet potato can give you clues of human movements.

> R. Day

Pigafetta, of Magellan's circumnavigation, wrote about sweet potatoes in the Ladrones (Guam). The early Spaniards mention sweet potatoes as indigenous products of the Philippines. Indeed many of the "tribal" Filipinos have their own indigenous words for the sweet potato although the lowlanders mostly seem to have adopted Amerind "camote" from Spanish.

Regards,  
Paul Kekai Manansala

| 55|2002-07-25 09:36:07|Oscar Nalesini|R: R: [austrie] R: R: Kunlun|

>Again Mr. Nalesini, you seem to have taken time to go

>to the library to fill yourself in on details that  
>you

apparently were unfamiliar with before.

What I do not have, though I would really like to have, dear Mr Manasala, is precisely the time to go to the library and read these as well as other things.

What I know is the result of my former studies on the Greek and Latin sources, not of a quick survey. I don't usually reply promptly because I have also other things to do in my life, and also because I like to reflect before writing down my ideas. As everybody who made some research in his life knows, you cannot become familiar even with details in 2 or 3 days if you are not already acquainted with the sources!

>According to your own type of argument,

there is also not a "single

>proof" that cinnamon and cassia trade came

via India

>to Arabia as you suggest. Despite the fact that this  
>would be much easier to ascertain than a southern route.

Sorry, but this is a complete misunderstanding. I never suggested that cinnamon and cassia came from India to Arabia, but that in the Greek geographical thought there are authors supporting an Arabian origin (Herodotus and Mela, for instance), others an African origin, and a third (smaller) group speaking on an Indian origin. What I actually support, is that according to the Greek and Latin authors, cassia and cinnamon came from the Horn of Africa or close by. There is no textual nor archaeological evidence of a south-eastern African provenance.

Again, you are confusing between data and interpretation.

>The fact that you don't know about Yueh

traders in "Greek

>times" again shows that you want me to provide

references

>that you are unaware of.

Please!! I said "there is no textual evidence about Yueh traders beyond the South China Sea in the time of the Greek sources we are discussing". Your critics are most welcome if they focus on what I really meant.

I didn't understand you are the keeper of top-secret informations! I believed that tools like a mailing list serve above all to discuss and exchange informations. I declared the references supporting my view, as the only correct way to discuss is to allow everybody to check my own readings, and criticize them. As you clearly explained me, I was wrong: this is not the way followed in this mailing list; I'm extremely sorry for this misunderstanding.

>Seafaring experts like Hornell and Haddon

had long noted

>that outrigger ships like the mtepe of East Africa  
>were probably derived from Austronesian ships. These may

have

>received first mention in the Periplus which talks  
>of boats

sewn with coconut fiber (as with the mtepe).

This is a well known fact. Actually I think outrigger is a better argument than sewing, because sewn boat technology is more widespread. The same Periplus mentions special sewn boats from Ommana (apparently not far from Hormuz) called "madarate".

There are recent archaeological finds (see also my first post) suggesting that bitumen-coated reed-bundle boats were in use at least since the V millennium BC in the Persian Gulf, and by the middle of the III the inhabitants of coastal Oman had open sea going boats made also with lashed wooden planks. Lashed wooden planks is also the technology of the Egyptian ships, and its possibly oldest existing example is the boat of Khufu, now dated ca. 2650 BC. These may also be the ancestors of the later sewn boats of the seas around Arabia.

The time the E african boats started to have double outriggers is undetermined. They have been convincingly compared to SE Asian similar crafts; however, Hornell describes an autonomous process of transformation of balsa stabilisers into double outriggers in a river of Colombia, suggesting that the double outrigger may result also from independent invention (Some American balanced conoes: stages in the invention of the double outrigger, Man, 1928, p. 129-33. Ghosh! Now you know my secret).

Just for the sake of precision: the Periplus does not say which fiber the people of Rhapta used to sew the boats; it just says they had sewn boats. Actually, I think a Greek word for "coconut" didn't exist at all.

>Of course, the coconut of East Africa is

also thought

>to have been brought by Austronesians. Aloe wood was  
>a direct product of SE Asia, and in Muslim times tortoise  
>shell

was connected directly with SE Asia and it is

>even mentioned as a

product traded from there in African

>markets.

You are speaking about the introduction of coconut into Africa, which certainly came from SE Asia (and possibly banana too) in a still uncertain date. But this is not the point I commented on. In your former post you said that Rhapta exported coconuts and aloe wood according to the Greek sources, and this is not true: the Greek sources do not say it.

>You mention Herodotus obscure' references

to cinnamon

>as traded by the Arabians but even that early author  
>indicates that the exact source of Arabian cinnamon  
>was unknown

to him. "[3.111] Still more wonderful is

>the mode in which they collect

the cinnamon. Where the

>wood grows, and what country produces it, they

cannot

>tell - only some, following probability, relate that it  
>comes from the country in which Bacchus was brought up."

See above my answer to ">According to your own type of argument..."

>In latter times, it is clear that Greek and

Roman

>authors, as they became more familiar with

southeastern

>Africa, were aware that cinnamon and cassia came into  
>Arabian markets from Africa. Interesting how you try to  
>minimize

Ptolemy whose geographical knowledge was

>later and obviously

superior to the earlier writers.

Ptolemy does not locate Kinnamomophoros on the coast of SE Africa (and for SE Africa I mean today Tanzania, Mozambique, Madagascar), and there is no information on SE Africa before the middle of the 1st century AD in the graeco-roman literature.

It is not the case of Ptolemy, but in line of principle the knowledge of later writers is not necessarily (let alone "obviously") superior. It may be just different. Gervaise of Tilbury and Matthew Paris lived centuries later than Ptolemy, but according to our concept of geography their knowledge of the earth can hardly be considered superior.

>Ptolemy clearly locates major spice markets

on the coast

>of Africa as did later writers including Cosmas. Of course,  
>they could have only vague ideas where these Asian  
>spices were

coming from since they followed a southern

>oceanic route. If they

had come from Indian and Arabia,

>this should have been very clear, but

the writers

>instead thought spices in Arabian markets came to the

Arabians

>from Africa!

Again, you suppose that the spices were Asian, but the evidence is lacking. The trading route bringing the spices to the Mediterranean coast shifted: sometimes the commerce followed the Red Sea into Egypt, in other times the main routes were the Arabian caravan tracks into the Levant. This should be the reason why some authors say that the spices come from Arabia.

If the the mariners followed a southern trans-oceanic route in the I century AD, there should be cinnamon or cassia in Menouthias or Rhapta; and this is not the case.

>And, of course, they never place the Regio

cinnamomifera

>in Arabia or India. So where is your own "single

proof?"

Some of them did. See above my answer to ">According to your own type of argument..."

>Now if you do some more preliminary

research you will find

>that cassia and cinnamon were considered products

of

>areas east of modern India well into Muslim times. Cinnamon

>harvesting in Sri Lanka begins only after the importation

>of the

Tamil-speaking harvesting caste in the 13th

>century.

There is no question on the cinnamon traded in the 12th or 13th century. We were discussing the Greek and Roman sources. And as I already said, there is no way to prove (so far as I know, at least) that the Greek words 'kassia' and 'kinnamomon' referred to the spices traded in much later times under the same name.

>Well before this time southern traders were

dealing in

>spices like cassia, cinnamon and cloves with China.

>From the Chinese annals themselves it is clear that they

>had no

idea of cinnamon coming from India in early times.

>Both cinnamon and

cassia are products of the countries

>of the Nanyue (before and after

this region's conquest)

>or further south.

I didn't say that. I didn't even assert that there was no cinnamon (in our modern meaning of this word) trade in the South China Sea.

>I will wait for your answer after your next

library trip.

I never realized that it is a shame to check or read something in a library. On the contrary, this is something that some people should do more often.

Regards

Oscar Nalesini

| 56|2002-07-25 10:10:38|a.manansala@attbi.com|Re: R: R: [austrie] R: R: Kunlun|

Oscar Nalesini wrote:

> >The fact that you don't know about Yueh traders in "Greek  
> >times" again shows that you want me to provide references  
> >that you are unaware of.  
>  
> Please!! I said "there is no textual evidence about Yueh traders beyond the  
> South China Sea in the time of the Greek sources we are discussing". Your  
> critics are most welcome if they focus on what I really meant.

Well then could you at least admit there were Yueh traders  
during "Greek times" and that the Chinese knew cinnamon to  
come from countries of Nanyue (Guilin, for example) and  
further south?

>  
> >Seafaring experts like Hornell and Haddon had long noted  
> >that outrigger ships like the mtepe of East Africa  
> >were probably derived from Austronesian ships. These may have  
> >received first mention in the Periplus which talks  
> >of boats sewn with coconut fiber (as with the mtepe).  
>  
> This is a well known fact. Actually I think outrigger is a better argument than  
> sewing, because sewn boat technology is more widespread. The same Periplus  
> mentions special sewn boats from Ommana (apparently not far from Hormuz) called  
> "madarate".  
> There are recent archaeological finds (see also my first post) suggesting that  
> bitumen-coated reed-bundle boats were in use at least since the V millennium BC  
> in the Persian Gulf, and by the middle of the III the inhabitants of coastal  
> Oman had open sea going boats made also with lashed wooden planks. Lashed wooden  
> planks is also the technology of the Egyptian ships, and its possibly oldest  
> existing example is the boat of Khufu, now dated ca. 2650 BC. These may also be  
> the ancestors of the later sewn boats of the seas around Arabia.  
> The time the E african boats started to have double outriggers is undetermined.  
> They have been convincingly compared to SE Asian similar crafts;



It's not just the outrigger, but the sewing techniques and the lateen sails. The early Near Eastern and Mesopotamian craft had "square" sails. It is widely believed that the lateen sail came to this region as well as to Africa via Austronesian influence (Zabaj, Wak-wak?).

>

>

> >Of course, the coconut of East Africa is also thought  
> >to have been brought by Austronesians. Aloe wood was  
> >a direct product of SE Asia, and in Muslim times tortoise  
> >shell was connected directly with SE Asia and it is  
> >even mentioned as a product traded from there in African  
> >markets.

>

> You are speaking about the introduction of coconut into Africa, which certainly  
> came from SE Asia (and possibly banana too) in a still uncertain date. But this  
> is not the point I commented on. In your former post you said that Rhapta  
> exported coconuts and aloe wood according to the Greek sources, and this is not  
> true: the Greek sources do not say it.

>

Well, that's a matter of interpretation of the texts.

We do know from latter sources (including Muslim) that Rhapta (or the adjoining areas, Zanzibar, Pemba, etc.) was indeed an important market for coconut, aloe wood and tortoise shell products.

There are some who believe coconuts were mentioned even in ancient Egyptians texts (along with cassia and cinnamon).

> Ptolemy does not locate Kinnamomophoros on the coast of SE Africa (and for SE  
> Africa I mean today Tanzania, Mozambique, Madagascar),

He does however locate the important spice markets in or around Rhapta.

In this sense, he verifies what Pliny and other writers stated.

- > It is not the case of Ptolemy, but in line of principle the knowledge of later
- > writers is not necessarily (let alone "obviously") superior. It may be just
- > different. Gervaise of Tilbury and Matthew Paris lived centuries later than
- > Ptolemy, but according to our concept of geography their knowledge of the earth
- > can hardly be considered superior.
- >

Ptolemy's Geography is rather widely recognized as the premier "Greek" geographical work. It is, of course, the first to make use of the grid mapping system.

- >
- >>Ptolemy clearly locates major spice markets on the coast
- >>of Africa as did later writers including Cosmas. Of course,
- >>they could have only vague ideas where these Asian
- >>spices were coming from since they followed a southern
- >>oceanic route. If they had come from Indian and Arabia,
- >>this should have been very clear, but the writers
- >>instead thought spices in Arabian markets came to the Arabians
- >>from Africa!
- >
- > Again, you suppose that the spices were Asian, but the evidence is lacking. The
- > trading route bringing the spices to the Mediterranean coast shifted: sometimes
- > the commerce followed the Red Sea into Egypt, in other

times the main routes > were the Arabian caravan tracks into the Levant. This should be the reason why

- > some authors say that the spices come from Arabia.

The latter authors clearly outline that the spices came into southern Arabia from Africa. From there they went to Egypt, and then to Europe.

- > If the the mariners followed a southern trans-oceanic route in the I century AD,
- > there should be cinnamon or cassia in Menouthias or Rhapta; and this is not the

> case.

>

In fact, it is the case. Ptolemy clearly locates Azania to the south in the region of Rhapta. The Regio Cinnamomifera is universally placed either in Azania or south of Azania.

>>Now if you do some more preliminary research you will find  
>>that cassia and cinnamon were considered products of  
>>areas east of modern India well into Muslim times. Cinnamon  
>>harvesting in Sri Lanka begins only after the importation  
>>of the Tamil-speaking harvesting caste in the 13th  
>>century.

>

> There is no question on the cinnamon traded in the 12th or 13th century. We were  
> discussing the Greek and Roman sources. And as I already said, there is no way > to prove (so far as I know, at least)

that the Greek words 'kassia' and

> 'kinnamomon' referred to the spices traded in much later times under the same  
> name.

>

I don't know what you mean here. Pliny, Theophrastus and others give fairly detailed botanical descriptions of cinnamon and cassia, which they clearly differentiated from each other (and from malabathrum).

That they meant Indonesian cinnamon rather than Ceylon cinnamon seems rather clear as they imply or state that both the leaves and the bark have rather similar aromatic properties (as is the case with Cassia vera from Indonesia).

Regards,  
Paul Kekai Manansala

| 57|2002-07-25 11:44:29|TTT UUU|Re: origins of the sumerians|

*travelershome* wrote:

i am looking for the starting place of the sumerians or rather what they called themselves the sag-giga. so far this is what i have foudn. they came from the northern marshes of the caspian sea, from meotis. after they arrived in the tigris area, by their neighbors they lived in kengir-the land of many river. they called their language emegir. i have found a conflicting story about their language and what group it belongs with. one is austriac, the other ural altai. i have also found connections with ancient turanians origins the sumerian-scythian-hun-magyar identity and continuity . current language connected best is hungarian 51 of 53 grammar points connect. www.igs.net for that last bit. so here is my basic question- where did they come from, how did they seem to appear on the scene with such an advanced approach to existence. i have read that their religion says they came from another planet. i have read that the preceding elamite culture helped them develop theirs, and i have read that clearing the marshes and controlling the waters in that area sped them on their way towards what is now known as the most advanced culture in the ancient world. can anyone connected with this site assist me? pamela pallessen-scherehart

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Thor Heyerdahl investigated the origins of the Sumerians to a degree. It is written in his book I believe called... Voyage of the Tigris or something like that. He felt/concluded the Sumerians came to the delta areas from the sea, somewhere to the south in the Persian Gulf.

R. Day

---

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| 58|2002-07-25 11:53:25|TTT UUU|Re: South America|

Sweet Potatoes come from S. America. That is the home. In New Zealand they are called cu-mer-ah, in Korea they arrived from south Japan and are called go-gu-ma. The best guess is that the Polynesians moved them across the Pacific after obtaining them from Peru, Chile, Equador or Columbia. They do not grow easily in cold climates.

Easter Island was once forested with a Palm (now extinct) that was related to a palm from Chile-Peru. The movie Rappa-Nui portrays the cutting of the last palm. Thor Hayerdahl found several links (bilological and cultural) between Easter Island and S. America. It is also possible that the Polynesians of the Marquesses also knew of S. and Central America.

R. Day

*"pinatubo.geo"* wrote:

```
--- In austric@y..., TTT UUU wrote:
>
> The sweet potato made its way across the Pacific from S.
America so
either the S. Americans took it west or Polynesians (or
others)
arrived and brought it back west with them. The dispersal
of the sweet
potato can give you clues of human movements.
> R. Day

Pigafetta, of Magellan's circumnavigation, wrote about
sweet potatoes
in the Ladrones (Guam). The early Spaniards mention sweet
potatoes
as indigenous products of the Philippines. Indeed many of
the "tribal"
Filipinos have their own indigenous words for the sweet
potato although
the lowlanders mostly seem to have adopted Amerind "camote"
from Spanish.

Regards,
Paul Kekai Manansala
```

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| 59|2002-07-25 12:35:37|a.manansala@attbi.com|Re: South America|

>  
> Sweet Potatoes come from S. America. That is the home. In New Zealand they are  
> called cu-mer-ah, in Korea they arrived from south Japan and are called  
> go-gu-ma. The best guess is that the Polynesians moved them across the Pacific  
> after obtaining them from Peru, Chile, Equador or Columbia. They do not grow  
> easily in cold climates.

Yes there's no doubt that sweet potatoes originated in South America. Were sweet potatoes found in Japan and Korea before the European discovery of the "New World?"

Regards,  
Paul Kekai Manansala

| 60|2002-07-25 17:19:55|Jogesh Panda|Re: South America|

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

>  
> Yes there's no doubt that sweet potatoes originated  
> in  
> South America.

Is this true? I always thought that some species of sweet potato was native to the Gondwana tract[ancient/proto-historic Central India].

I satisfied myself with the following argument. In the Jagannath Temple in Puri, Orissa, post-Columbian food items [e.g. tomatoes or potatoes] are not offered to the deities, whereas sweet potatoes are offered. Jagannath has been traditionally associated with savaras, a tribe speaking an austro-asiatic language. Any thoughts on this?

Best.

Jogesh Panda

---

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| 61|2002-07-25 18:11:55|Juha Savolainen|9 questions for Oscar Nalesini|

9 Questions for Oscar Nalesini:

I have followed the debate between Oscar Nalesini and Paul Kekai Manansala with great interest, not least because my posts started the whole debate. I have questions for both contestants. As I have not posted any messages (after having raised the topic) I must make use of posts that were not directed to me. I hope that this is not seen objectionable. I re-enter the discussion by asking 9 questions, all meant this time primarily for Oscar Nalesini, but no harm will follow if Paul Kekai Manansala would comment on some of these questions, too.

(1) You wrote: ?There is no need to involve the presence of Austronesian or other eastern sailors in the Middle Eastern waters to explain the presence of such exotic goods in the II millennium BC Near Eastern sites, for the navigation capacity has had already attained at least a millennium before. These evidences rather suggest that in the II millennium BC (and we

don't know if for a while or for a longer period) the maritime trade circuits connected and allowed goods to travel from one edge of the ocean to the other: resin from Madagascar in Mesopotamia, and Indian goats in Timor.?

- Do you mean that you can present a plausible trade route for the cloves discovered from a Syrian site and dating back to 1700/1600 BCE that does not substantially involve Austronesian mariners? And if you do, could you perhaps give your candidate for the trade route and the mariners?

(2) You wrote: ?I don't think we will be able in the future to solve problems such as the origins of Malagasy language embracing hyper-diffusionistic ideas of Egyptians, Sumerians or Austronesians civilizing the entire world.?

- Given that you had read just four previous postings of the ?Austric? list, what were the posts you were referring to?

(3) You wrote: ?Names can move from one thing to another, especially if the latter substitutes the former. Do we have evidence that the Greeks and Roman "cinnamon" refers to the same botanic species the Middle Age Europeans bought in the Middle East, and later saw in the Indonesian archipelago?I don't know of any sample of cinnamon found in western Indian Ocean archaeological sites?Again, you suppose that the spices were Asian, but the evidence is lacking.?

- Do you think that we have no good evidence for believing that during classical antiquity Greece, Rome et al. participated in trade with ?genuine? cinnamon and cassia?

(4) You wrote: ?If these spices were traded directly into E Africa by Austronesian sailors, how is it possible that these commodities were available only on the Gulf of Aden...What I actually support, is that according to the Greek and Latin authors, cassia and cinnamon came from the Horn of Africa or close by. There is no textual nor archaeological evidence of a south-eastern African provenance??



- Do you believe that ?cassia? and ?cinnamon? were from the Horn of Africa? And if so, could you give your views about their real nature?

(5) You wrote: ?The Periplus Maris Erythraei. Chapter 17 says: "[...] the area [of Rhapta] exports a great amount of ivory [...], rhinoceros horn, best quality tortoise shell after the Indian (i.e. this is a local quality), a little nautilus shell" (Casson's translation). No cassia, no cinnamon, nor other spices. This fits with the informations of other Greek and Egyptian sources about the north-eastern african origins of both cassia and cinnamon? ?

- Do you suggest that whatever ?cinnamon? and ?cassia? might have meant, there is no evidence in classical sources that Eastern Africa was involved in trading them?

(6) You wrote: ?It was technically possible for the Austronesian sailors to trade with Eastern Africa, and probably they did (how else can we explain Madagascar?), but there is no evidence of their presence in the historical records for the "Roman" or earlier times, and above all there is no evidence that the most famous spices of the Erythraean Sea commerce were Southeast Asia's? So far as I know, there is no textual evidence about Yueh traders beyond the South China Sea in the time of the Greek sources we are discussing. ?

- Do you suggest that it is unproblematically true that the Greek and Roman sources did not refer to Austronesians when they discussed the inhabitants of Eastern Africa? If so, what might be the textual and other arguments?

(7) You wrote: ?The Kunlun are known from much later chinese sources (VII cent. AD, is it correct?), and are generally considered the coastal inhabitants of the Malay world. But I don't see any proof that they were the only people able to navigate in the Indian Ocean, nor that they moored on the Near Eastern shores at least 2 millennia before.?

- Who claimed that the Kunlun were the 'only people able to navigate in the Indian Ocean'? And what sort of evidence would persuade you to take seriously the possibility that Austronesian mariners visited Eastern Africa, say, before 1000 BCE?

(8) You wrote: 'No text mentions exports of coconuts and aloe from Rhapta in Roman times.'

- Do you think that this textual argument is non-problematic?

(9) You wrote: 'If the the mariners followed a southern trans-oceanic route in the I century AD, there should be cinnamon or cassia in Menouthias or Rhapta; and this is not the case.'

- Do you mean 'cinnamon and cassia' or 'cinnamon' and 'cassia'? or both? And would the discovery of either make you change your views on the possibility of Austronesian dominated trade of cinnamon, cassia and cloves trade?

Many thanks for answers.

Best regards, Juha Savolainen

---

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| 62|2002-07-25 22:39:10|a.manansala@attbi.com|Re: South America|

>

> --- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

>

>>

>> Yes there's no doubt that sweet potatoes originated

>> in

>> South America.

>

> Is this true? I always thought that some specis of

> sweet potato was native to the Gondwana

> tract[ancient/proto-historic Central India].  
>

I haven't heard of this theory. Do you have any refs.

Regards,  
Paul Kekai Manansala

| 63|2002-07-26 00:46:48|Jogesh Panda|Re: Sweet Potato|

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

> I haven't heard of this theory. Do you have any  
> refs.  
>  
>

Purseglove, J.W. 1968. Tropical Crops. Dicotyledons.  
2. London, Longman.  
Purseglove, J.W. 1972. Tropical Crops: Monocotyledons.  
London, Longman.

According to this authority, some specis of root crops  
[yam and sweet potato] developed independently of each  
other in the continents of Asia,Africa and South  
America.

Dioscorea alata, Dioscorea esculenta and Dioscorea  
opposita [specis of yam/sweet potato] originated in  
South Asia.

Best.

Jogesh Panda

---

Do You Yahoo!?  
Yahoo! Health - Feel better, live better  
<http://health.yahoo.com>

| 64|2002-07-26 05:53:28|a.manansala@attbi.com|Re: Sweet Potato|

>  
> --- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:  
>  
> > I haven't heard of this theory. Do you have any  
> > refs.  
> >  
> >  
> >  
> >  
> Purseglove, J.W. 1968. Tropical Crops. Dicotyledons.  
> 2. London, Longman.  
> Purseglove, J.W. 1972. Tropical Crops: Monocotyledons.  
> London, Longman.  
>  
> According to this authority, some specis of root crops  
> [yam and sweet potato] developed independently of each  
> other in the continents of Asia,Africa and South  
> America.  
>  
> Dioscorea alata, Dioscorea esculenta and Dioscorea  
> opposita [specis of yam/sweet potato] originated in  
> South Asia.  
>  
>

There's no doubt that many yam species (Dioscorea) evolved  
in Asia and Africa.

However, I think the standard opinion is that Ipomoea  
batatas, which is what we refer to usually when we say  
"sweet potato" is of South American origin.

Regards,  
Paul Kekai Manansala

| 65|2002-07-26 06:36:45|a.manansala@attbi.com|Re: 9 questions for Oscar Nalesini|

> 9 Questions for Oscar Nalesini:  
>  
>

I'd like to comment on just a few of the questions.

- > (3) You wrote: "Names can move from one thing to
- > another, especially if the latter substitutes the
- > former. Do we have evidence that the Greeks and Roman
- > "cinnamon" refers to the same botanic species the
- > Middle Age Europeans bought in the Middle East, and
- > later saw in the Indonesian archipelago? I don't know
- > of any sample of cinnamon found in western Indian
- > Ocean archaeological sites? Again, you suppose that the
- > spices were Asian, but the evidence is lacking.?"
- >
- > - Do you think that we have no good evidence for
- > believing that during classical antiquity Greece, Rome
- > et al. participated in trade with "genuine" cinnamon
- > and cassia?
- >

The evidence of spices is generally lacking regardless of the origin. However, we do have tidbits that suggest Asian spices. The cloves have already been mentioned. Also, camphor from Borneo has been tested dating to 200 BCE.

The big problem of course is that spices were consumables and perishable.

We have botanical descriptions and a pretty good agreement between different sources -- Chinese, Arab, etc. that consistently describe cinnamon and cassia through the ages. There is also of course the argument that the words "cinnamon" and "cassia" both derive from the Proto-Austronesian word for "bark."

If I remember correctly, it was Strabo who first theorized that the cinnamon and cassia from African markets actually originated from somewhere in Asia.

- > (8) You wrote: "No text mentions exports of coconuts
- > and aloe from Rhapta in Roman times.?"
- >
- > - Do you think that this textual argument is

> non-problematic?

>

A rather difficult problem. I believe it was Schoff who first suggested that coconuts and aloes were mentioned in Periplus. He was followed of course by Miller.

The first Portuguese to visit this region do mention that the native boats were sewn with coconut fibers.

Hugh Harries, a leading expert on coconuts, has suggested that ports like Rhapta needed coconut trees to be established in the first place. The palms made human habitation in these areas possible by acting as natural desalinization plants.

Harries maintains an interesting yahoo group at:

<http://groups.yahoo.com/group/ICCRA/>

Regards,

Paul Kekai Manansala

<http://home.attbi.com/~a.manansala/afro.htm>

| 66|2002-07-27 06:34:10|Oscar Nalesini|R: [austriac] 9 questions for Oscar Nalesini|

(1) You wrote: ?There is no need to involve the presence of Austronesian or other eastern sailors in the Middle Eastern waters to explain the presence of such exotic goods in the II millennium BC Near Eastern sites, for the navigation capacity has had already attained at least a millennium before. These evidences rather suggest that in the II millennium BC (and we don't know if for a while or for a longer period) the maritime trade circuits connected and allowed goods to travel from one edge of the ocean to the other: resin from Madagascar in Mesopotamia, and Indian goats in Timor.?

- Do you mean that you can present a plausible trade route for the cloves discovered from a Syrian site and dating back to 1700/1600 BCE that does not substantially involve Austronesian mariners? And if you do, could you perhaps give your candidate for the trade route and the mariners?

---

I meant that the people of Mesopotamia, the Persian Gulf and the Indus Valley owned the technology to navigate the Arabian Sea at least from the preceding millennium, so that until some evidence of

Austronesian presence will be discovered in the Middle East, we can assume that the cloves arrived in Mesopotamia on board of Indian, Arabian or Mesopotamian ships. Who brought the cloves until the eastern or southern terminal(s) of these ships, and where this terminal(s) was, is just matter of speculation.

I don't consider cloves an evidence of Austronesian presence, because commodities may travel independently from people. The quantity of cloves so far discovered in the Middle East accounts only for an episodic trade of this commodity.

(2) You wrote: ?I don't think we will be able in the future to solve problems such as the origins of Malagasy language embracing hyper-diffusionistic ideas of Egyptians, Sumerians or Austronesians civilizing the entire world.?

- Given that you had read just four previous postings of the ?Austric? list, what were the posts you were referring to?

---

Not one post in particular, but a more general idea that I felt from your correspondence with Paul K. Manasala. Actually the first post I red refers also to Eden in the East, and Oppenheimer actually follow a diffusionistic perspective.

(3) You wrote: ?Names can move from one thing to another, especially if the latter substitutes the former. Do we have evidence that the Greeks and Roman "cinnamon" refers to the same botanic species the Middle Age Europeans bought in the Middle East, and later saw in the Indonesian archipelago?I don't know of any sample of cinnamon found in western Indian Ocean archaeological sites?Again, you suppose that the spices were Asian, but the evidence is lacking.?

- Do you think that we have no good evidence for believing that during classical antiquity Greece, Rome et al. participated in trade with ?genuine? cinnamon and cassia?

---

I think we have no evidence at all. We have two names of spices that according to the Greek authors came from the Horn of Africa, and cinnamon mostly from the interior, not from the coast. As I said, there were also others opinions among the Greek geographers; this is a longer argument, but in short it is possible to explain these different opinions mostly with the shifting of the trade routes through the time, and demonstrate that cassia and cinnamon ultimately came out of Africa.

Pliny (XII 89-95 (42)) has quite a long description of the cinnamon plant, in addition to which he explains how people cropped it, and describes even the religious rituals performed by the people during the harvesting season. He placed this area in Africa, and I don't see any reason to believe that he confused Africa with SE Asia. Pliny barely knew that there was something beyond India, so it is not reasonable to suppose that his sources reported something they saw in SE Asia. From his evidence we can only conclude that people harvested cinnamon in Africa; so, it was an African plant. Which one, I don't know.

(4) You wrote: ?If these spices were traded directly into E Africa by Austronesian sailors, how is it

possible that these commodities were available only on the Gulf of Aden...What I actually support, is that according to the Greek and Latin authors, cassia and cinnamon came from the Horn of Africa or close by. There is no textual nor archaeological evidence of a south-eastern African provenance??

- Do you believe that ?cassia? and ?cinnamon? were from the Horn of Africa? And if so, could you give your views about their real nature?

---

To the first question my answer is yes. To the second: no, I cannot because nobody identified any african botanical species which might be the origin of cassia and cinnamon. I'm perfectly aware that this is the weakest point of my opinion: if there is no african plant giving something that might be cinnamon, the only reasonable explanation is an Asian import. However, Greek and Latin sources knew that cinnamon grew in Africa, not that it was just sold there. Besides this, as I said above, if cinnamon arrived in Africa through a southern oceanic route, I don't understand why cinnamon is not recorded in the marts south of today Somalia. See also above (no 4) for Pliny's evidence.

(5) You wrote: ?The Periplus Maris Erythraei. Chapter 17 says: "[...] the area [of Rhapta] exports a great amount of ivory [...], rhinoceros horn, best quality tortoise shell after the Indian (i.e. this is a local quality), a little nautilus shell" (Casson's translation). No cassia, no cinnamon, nor other spices. This fits with the informations of other Greek and Egyptian sources about the north-eastern african origins of both cassia and cinnamon? ?

- Do you suggest that whatever ?cinnamon? and ?cassia? might have meant, there is no evidence in classical sources that Eastern Africa was involved in trading them?

---

Yes, this is exactly what I meant. Let's say Southeastern Africa, to avoid confusion with Somalia, for instance.

(6) You wrote: ?It was technically possible for the Austronesian sailors to trade with Eastern Africa, and probably they did (how else can we explain Madagascar?), but there is no evidence of their presence in the historical records for the "Roman" or earlier times, and above all there is no evidence that the most famous spices of the Erythraean Sea commerce were Southeast Asia's? So far as I know, there is no textual evidence about Yueh traders beyond the South China Sea in the time of the Greek sources we are discussing. ?

- Do you suggest that it is unproblematically true that the Greek and Roman sources did not refer to



Austronesians when they discussed the inhabitants of Eastern Africa? If so, what might be the textual and other arguments?

---

There is not a word in Greek and Latin sources on people arriving in Eastern Africa coming from the other side of the Indian Ocean. The only possibility is to find possible austronesian roots in the place names of Azania. Unfortunately, most of the names of coastal sites of Azania do not reproduce the local name: they are Greek names given to African places for the benefit of Roman empire subjects (Rhapta is a case in point).

(7) You wrote: ?The Kunlun are known from much later chinese sources (VII cent. AD, is it correct?), and are generally considered the coastal inhabitants of the Malay world. But I don't see any proof that they were the only people able to navigate in the Indian Ocean, nor that they moored on the Near Eastern shores at least 2 millennia before.?

- Who claimed that the Kunlun were the ?only people able to navigate in the Indian Ocean?? And what sort of evidence would persuade you to take seriously the possibility that Austronesian mariners visited Eastern Africa, say, before 1000 BCE?

---

I exaggerated in the answer, but I got the impression that this was what you basically supported when you assumed Solheim's Nusantara traders as the men who possibly brought the cloves all the way to Mesopotamia. As I said in my first post, and also above here, the Near Eastern people were able to sail on the Ocean at least from the III millennium BC. We have archaeological evidence of this, but no evidence of people coming from some place east of India into Middle Eastern waters.

(8) You wrote: ?No text mentions exports of coconuts and aloe from Rhapta in Roman times.?

- Do you think that this textual argument is non-problematic?

---

It is a fact: the only two Greek sources mentioning Rhapta (Periplus and Ptolemy) do not record aloe or coconuts. Please, understand me, because this seems actually the origin of a lot of misunderstanding in our conversation: I don't mean they didn't exist in or near Rhapta at that time: only that the Roman time sources do not mention them (this difference is basic).

(9) You wrote: ?If the the mariners followed a southern trans-oceanic route in the I century AD, there should be cinnamon or cassia in Menouthias or Rhapta; and this is not the case.?

- Do you mean ?cinnamon and cassia? or ?cinnamon? and ?cassia? ? or both? And would the discovery of either make you change your views on the possibility of Austronesian dominated trade of cinnamon, cassia and

cloves trade?

---

I mean both.

For the second question the answer is, obviously, yes. I probably gave the impression to fight for the sole true faith! I assure everybody that this is not the case; otherwise I wouldn't have subscribed to this mailing list.

But I dislike to reconstruct ancient history basing only on suppositions. I understand that this way to proceed is much less exciting, but it is closer to the real documentation.

Many thanks to you

Best regards

Oscar Nalesini

| 67|2002-07-27 06:34:10|Oscar Nalesini|R: R: R: [austric] R: R: Kunlun|

> Well then could you at least admit there were Yueh traders

> during "Greek times" and that the Chinese knew cinnamon to  
> come from countries of Nanyue (Guilin, for example) and  
>

further south?

Of course! I never said the contrary. I always discussed the documents written in Greek and Latin. But I still reject you assertion that we have evidence of their presence in E Africa. And I don't remember even Chinese sources saying that.

> Well, that's a matter of interpretation of the texts.

> We do know from latter sources (including Muslim) that

> Rhapta (or the adjoining areas, Zanzibar, Pemba, etc.)  
> was indeed an important market for coconut,  
> aloe wood and

tortoise shell products.

> There are some who believe coconuts were mentioned even

> in ancient Egyptians texts (along with cassia and  
> cinnamon).

Sorry, but the "coconut and aloe" question is not matter of interpretation. I checked the Greek text (I confess: yesterday morning I was able to spend some time in a library, for I don't own a copy of Ptolemy). Ptolemy has only the coordinates of Rhapta, of its river and the promontory, but doesn't say anything about the trade of Rhapta. The sole list of Rhapta area exports dating to the Roman empire time is in the Periplus. I already transcribed this list in an earlier post, and Juha Savolainen now reproduces it in his own. There are stops [...] in my transcription, but I didn't omit anything from the list of merchandises; in that place there is a comment on the ivory (inferior to that of Adulis) which I considered superfluous to

copy.

You may be right assuming that the merchandises of the later period were traded also in the Roman time, but the roman sources definitely do not say this. The same for the sewn boat. In the description of Menouthias island the Periplus says that the people use sewn boat and dugout crafts for fishing and catching turtles (ch. 15), and about Rhapta that the town derived his name from the boats (rhapta means sewn) (ch. 16), and that's all. We may reasonably suggest that they were sewn using coconut fibres, as Shoff did, but this is not in the Greek text.

We can also suppose either that the sources omit coconut and aloe because they were not available at Rhapta, or because the Romans were not interested in (for whatever reason), but these are just hypotheses.

There are many Egyptian word whose meaning we ignore. There is no coconut in the Deir el-Bahari reliefs of the Punt expedition. And still, today coconuts grow as north as Dhofar, in Oman. So, even if the Egyptian knew the coconut, it does not mean that it came from SE Africa.

> He does however locate the important spice

markets in

> or around Rhapta.

> In this sense, he verifies what

Pliny and other writers

> stated.

Ptolemy plotted some places on the Azania coast; however he gave only the coordinates of these places, without description. We can reasonably presume that these places traded with the Red Sea too, but what they exported is not listed by Ptolemy. Pliny and earlier authors always refer to Eritrea and Somalia.

> The latter authors clearly outline that the spices

> came into southern Arabia from Africa. From there they

>

went to Egypt, and then to Europe.

No objection for the continent. We disagree on which part of Africa the greek and Latin sources referred to.

> In fact, it is the case. Ptolemy clearly

locates

> Azania to the south in the region of Rhapta. The

> Regio

Cinnamomifera is universally placed either in

> Azania or south of

Azania.

The Periplus and Ptolemy say that Azania is the African coast from Opone/Aromata Cape down to Rhapta and Prason, farther south. So this region was actually much wider than just the area around Rhapta. According to Ptolemy, however, the Cinnamon country was not on the coast. He does not give coordinates, but says that it is close to the Nile (IV 7,10). Strabo had almost the same view in XVI 4,14, where, describing the coast between Bab al-Mandeb and Cape Guardafui, says:

"And one comes to several river-lands in succession that produce frankincense along the rivers, and to rivers that extend as far as the Cinnamon-bearing country; [...] Then to another river and to the Daphnus Harbour and to the River-land of Apollo, as it is called, which produces, in addition to frankincense, both myrrh and cinnamon; but the cinnamon is more abundant in the neighbourhood of the places that are deep in the interior." (Jones' translation)

What you can see here, is that for Strabo the cinnamon came from the same country (or very close to) of frankincense and myrrh, which don't grow in SE Africa, but only in Eritrea, northern Somalia and Southern Arabia. Moreover, most of the cinnamon came from the interior rather than the coast. And Ptolemy essentially followed him.

> > There is no question on the cinnamon

traded in the 12th or 13th century. We were

> > discussing the Greek

and Roman sources. And as I already said, there is no way

> > to prove

(so far as I know, at least) that the Greek words 'kassia' and

> >

'kinnamomon' referred to the spices traded in much later times under the same

> > name.

> I don't know what you mean here. Pliny, Theophrastus and others

> give fairly detailed botanical descriptions of

cinnamon

> and cassia, which they clearly differentiated from

>

each other (and from malabathrum).

> That they meant Indonesian

cinnamon rather than Ceylon

> cinnamon seems rather clear as they imply

or state that

> both the leaves and the bark have rather similar

>

aromatic properties (as is the case with Cassia vera

> from

Indonesia).

I mean that the same word may change meaning. Take as an example the italian word 'libro' (book) which derives from the latin 'liber'. The word is the same, but they mean two different (though closely related) items: the italian word refers to the book as we conceive it today, while the latin word refers to the manuscript scroll. The latin word for the object made of written pages bounded together with a cover (as our books today) is 'codex'.

So, what I want to say is: given that the Greeks and Romans knew only African cassia and cinnamon (I know you don't agree on this point; but see also my answer to Juha Savolainen), and given that in the Middle ages Europeans markets received cassia and cinnamon only from Asia, it is fairly possible that the same word, at a given moment, shifted from the african to the asian product.

Best regards

Oscar Nalesini

| 68|2002-07-27 07:10:29|a.manansala@attbi.com|Re: R: R: R: [austric] R: R: Kunlun|

> > Well, that's a matter of interpretation of the texts.

>

> > We do know from latter sources (including Muslim) that

> > Rhapta (or the adjoining areas, Zanzibar, Pemba, etc.)

> > was indeed an important market for coconut,

> > aloe wood and tortoise shell products.

>

> > There are some who believe coconuts were mentioned even

> > in ancient Egyptians texts (along with cassia and

> > cinnamon).

>

> Sorry, but the "coconut and aloe" question is not matter of interpretation. I

> checked the Greek text (I confess: yesterday morning I was able to spend some

> time in a library, for I don't own a copy of Ptolemy).

Since the text contains arcane words it is certainly a matter of interpretation. We have the same case with the Egyptian words that were translated by the ancient Greeks as "cinnamon" and "cassia".

> There are many Egyptian word whose meaning we ignore. There is no coconut in the  
> Deir el-Bahari reliefs of the Punt expedition.

Again, this is just an opinion and not even one that originates from a botanist.

Maybe you should take the question to coconut discussion group that I listed which is populated by experts.

The identification of the palms on the Deir el-Bahri relief as doum types simply does not jibe with known facts or with Egyptian convention.

The doum is an aerial branching tree with palmate leaves and rather upright main trunk. This is how they are depicted in Egyptian art through the millennia.

The trees in the Deir el-Bahri relief are like the coconut palm with long leaves and leaning, non-aerial-branching trunks.

>

> > He does however locate the important spice markets in  
> > or around Rhapta.  
> > In this sense, he verifies what Pliny and other writers  
> > stated.

>

> Ptolemy plotted some places on the Azania coast; however he gave only the  
> coordinates of these places, without description. We can reasonably presume that >

these places traded with the Red Sea too, but what they exported is not listed

> by Ptolemy. Pliny and earlier authors always refer to Eritrea and Somalia.  
>

Ptolemy states quite clearly that these ports were markets for spices and aromatics. From other sources including Pliny it is clear that these spices included cinnamon and cassia that were traded further north eventually reaching Europe.

You seem to have reduced your argument to finding the one point that is not "provable" and then shifting your argument to this point.

But the fact is that whatever theory one takes it remains just that -- a theory. Your idea now that cinnamon and cassia had different meanings to the ancient Greeks (as compared to the Romans, Arabs?) is also simply conjecture.

>  
> The Periplus and Ptolemy say that Azania is the African coast from Opone/Aromata  
> Cape down to Rhapta and Prason, farther south. So this region was actually much  
> wider than just the area around Rhapta. According to Ptolemy, however, the  
> Cinnamon country was not on the coast. He does not give coordinates, but says  
> that it is close to the Nile (IV 7,10). Strabo had

almost the same view in XVI > 4,14, where, describing the coast between Bab al-Mandeb and Cape Guardafui,

> says:  
>  
> "And one comes to several river-lands in succession that produce frankincense  
> along the rivers, and to rivers that extend as far as the Cinnamon-bearing  
> country; [...] Then to another river and to the Daphnus Harbour and to the  
> River-land of Apollo, as it is called, which produces, in addition to  
> frankincense, both myrrh and cinnamon; but the cinnamon is more abundant in the  
> neighbourhood of the places that are deep in the interior." (Jones' translation)  
>  
> What you can see here, is that for Strabo the cinnamon came from the same  
> country (or very close to) of frankincense and myrrh, which don't grow in SE  
> Africa, but only in Eritrea, northern Somalia and Southern Arabia. Moreover,  
> most of the cinnamon came from the interior rather than the coast. And Ptolemy  
> essentially followed him.

>

Well this is contradicted, of course, with Pliny's description of cinnamon coming from trade that involved long journeys across the sea.

Of course, the ancient writers were now speculating about regions which they admitted to having little first-hand knowledge. So there is bound to be some confusion.

Btw, what spices do you believe were meant when the terms "cinnamon" and "cassia" were used? Have these been found in archaeological digs in Greece and Rome?

> I mean that the same word may change meaning. Take as an example the italian  
> word 'libro' (book) which derives from the latin 'liber'. The word is the same,  
> but they mean two different (though closely related) items: the italian word > refers to the book  
as we conceive it today, while the latin word refers to the  
> manuscript scroll.

They could have changed meaning but you have not really given us any reason to believe they did. As I noted, the ancient and medieval botanical descriptions of cinnamon and cassia happen to match, sometime rather precisely, the Asian species. Do you feel this is coincidence? And when do you feel the 'switch' in meanings occurred?

Note that the early Hebrew commentators described cinnamon and cassia as products of Asia, in reference to the Biblical words "kiddah," "ketzioth" and "kinamon."

Regards,  
Paul Kekai Manansala

| 69|2002-07-27 07:13:58|a.manansala@attbi.com|Re: R: [austriac] 9 questions for Oscar Nalesini|



- > But I dislike to reconstruct ancient history basing only on suppositions. I
- > understand that this way to proceed is much less exciting, but it is closer to
- > the real documentation.
- >

In that sense, is there any mention by ancient or medieval writers in the 'switch' in meanings of "cinnamon" and "cassia?"  
Is there any hard evidence, or is this just conjecture?

Regards,  
Paul Kekai Manansala

| 70|2002-07-27 13:41:44|Oscar Nalesini|R: R: R: R: [austrie] R: R: Kunlun|

> Sorry, but the "coconut and aloe" question is not matter of interpretation. I

> checked the Greek text (I confess:

yesterday morning I was able to spend some > time in a library, for I don't own a copy of Ptolemy).

Since the text contains arcane words it is certainly a matter of interpretation. We have the same case with the Egyptian words that were translated by the ancient Greeks as "cinnamon" and "cassia".

---

There is no arcane word in the text. The Periplus simply says nothing about the flora of Rhapta, nor on the fibres used to sew the boats.

> Ptolemy plotted some places on the Azania coast; however he gave only the

> coordinates of these places, without

description. We can reasonably presume that > these places traded with the Red Sea too, but what they exported is not listed

> by Ptolemy. Pliny

and earlier authors always refer to Eritrea and Somalia.

>

Ptolemy states quite clearly that these ports were markets for spices and aromatics. From other sources including Pliny it is clear that these spices included cinnamon and cassia that were traded further north eventually reaching Europe.

You seem to have reduced your argument to finding the one point that is not "provable" and then shifting your argument to this point.

---

I reduced nothing. Ptolemy's description of the Azania coast is just a list of places and their coordinates. The cape of Aromata is Guardafui or Hafun, and the markets where cassia and cinnamon were traded are on the Gulf of Aden, not in Azania.

Well this is contradicted, of course, with Pliny's description of cinnamon coming from trade that involved long journeys across the sea.

---

I already criticised your interpretation of Pliny XII 86-88 in my post of 21 July, and showed that you omitted precisely the sentences that didn't fit your reconstruction.

Of course, the ancient writers were now speculating about regions which they admitted to having little first-hand knowledge. So there is bound to be some confusion.

---

This is the reason why the detailed description of the cinnamon harvest given by Pliny cannot refer to a poorly known land.

Btw, what spices do you believe were meant when the terms "cinnamon" and "cassia" were used? Have these been found in archaeological digs in Greece and Rome?

---

If they had been found, there would be no room for this discussion.

They could have changed meaning but you have not really given us any reason to believe they did. As I noted, the ancient and medieval botanical descriptions of cinnamon and cassia happen to match, sometime rather precisely, the Asian species. Do you feel this is coincidence? And when do you feel the 'switch' in meanings occurred?

---

The reason is that there is no evidence that the spices traded in the Roman Empire came from SE Asia. The time this switch might have happened is between the Late Antiquity and the Middle Ages: I cannot be more precise.

Regards

Oscar Nalesini

| 71|2002-07-28 13:45:05|a.manansala@attbi.com|Re: R: R: R: R: [austric] R: R: Kunlun|

Oscar wrote:

>  
> > Ptolemy plotted some places on the Azania coast; however he gave only the  
> > coordinates of these places, without description. We can reasonably presume  
> that >  
> these places traded with the Red Sea too, but what they exported is not listed  
> > by Ptolemy. Pliny and earlier authors always refer to Eritrea and Somalia.  
> >  
>  
> Ptolemy states quite clearly that these ports were markets  
> for spices and aromatics. From other sources including Pliny  
> it is clear that these spices included cinnamon and cassia  
> that were traded further north eventually reaching Europe.  
>  
> You seem to have reduced your argument to finding the one  
> point that is not "provable" and then shifting your argument  
> to this point.  
> \_\_\_\_\_  
>  
> I reduced nothing. Ptolemy's description of the Azania coast is just a list of  
> places and their coordinates. The cape of Aromata is Guardafui or Hafun, and the  
> markets where cassia and cinnamon were traded are on the Gulf of Aden, not in  
> Azania.  
>  
>

Well, my view is that your argument has shifted. Others can make up their own minds. At first, you seemed to insist that the trade in spices and other "exotic" goods came through India. Now you are saying that there indeed was an important spice trade with Africa, but that the terminology has shifted.

Here is an excerpt from your first post:

"As you can see, there is no need to involve the presence of Austronesian or other eastern sailors in the Middle Eastern waters to explain the presence of such exhotic goods in the II millennium

BC Near Eastern sites, for the navigation capacity has had already attained at least a millennium before.

These evidences rather suggest that in the II millennium BC (and we don't know if for a while or for a longer period) the maritime trade circuits connected and allowed goods to travel from one edge of the ocean to the other: resin

from Madagascar in Mesopotamia, and Indian goats in Timor. What is clear, however, is that the maritime people from both the western and the Eastern Indian Ocean mastered the technology of the Ocean navigation, and were able to contact each other.

I don't think we will be able in the future to solve problems such as the origins of Malagasy language embracing hyper-diffusionistic ideas of Egyptians, Sumerians or Austronesians civilizing the entire world."

The coordinates given by Ptolemy show an African coastal location for Aromata that was significantly south of Rhapta:

Opone market place 81°00' 4"15

Sarapionis station and emporium 74°00' 3"00 S

Tonice market place 73°00' 4"15 S

Rhapta, metropolis of Barbaria, a short distance from the sea 71°00' 7"00

Avalites market place 74°00' 8"25

Malao market place 75°00' 6"30

Mundu market place 78°00' 7"00

Mosylum promontory and market place 79°00' 9"00

Cobe market place 80°00' 8"00

Acanna market place 82°00' 7"00

Aromata promontory and market place 83°00' 6"00

From Ptolemy's Geography, Book IV, Chapter 7

> Well this is contradicted, of course, with Pliny's

> description of cinnamon coming from trade that involved

> long journeys across the sea.

> \_\_\_\_\_

>

> I already criticised your interpretation of Pliny XII 86-88 in my post of 21

> July, and showed that you omitted precisely the sentences that didn't fit you

> reconstruction.

>

It really doesn't matter. Which ever interpretation you take, the fact is that cinnamon was seen as coming from trade that involved long sea voyages.

> Btw, what spices do you believe were meant when the  
> terms "cinnamon" and "cassia" were used? Have these  
> been found in archaeological digs in Greece and Rome?

> \_\_\_\_\_

>

> If they had been found, there would be no room for

this discussion.

>

So then, you also have no "single proof."

>

>

> They could have changed meaning but you have not really  
> given us any reason to believe they did. As I noted,  
> the ancient and medieval botanical descriptions of  
> cinnamon and cassia happen to match, sometime rather  
> precisely, the Asian species. Do you feel this is  
> coincidence? And when do you feel the 'switch' in  
> meanings occurred?

> \_\_\_\_\_

>

> The reason is that there is no evidence that the spices traded in the Roman  
> Empire came from SE Asia.

There is no \*hard\* evidence at all of where the spices referred to as cinnamon and cassia in the Roman empire came from whether it be Asia, Africa or elsewhere.

So that would apply equally to your terminology switch theory.

Regards,  
Paul Kekai Manansala

| 72|2002-07-30 07:29:18|Juha Savolainen|Comments for Oscar Nalesini|

Dear Oscar Nalesini,

Many thanks for your clarifications. To make my comments easily understandable, I include my questions here.

(1) My question: Do you mean that you can present a plausible trade route for the cloves discovered from a Syrian site and dating back to 1700/1600 BCE that does not substantially involve Austronesian mariners? And if you do, could you perhaps give your candidate for the trade route and the mariners?

Your reply: ?I meant that the people of Mesopotamia, the Persian Gulf and the Indus Valley owned the technology to navigate the Arabian Sea at least from the preceding millennium, so that until some evidence of Austronesian presence will be discovered in the Middle East, we can assume that the cloves arrived in Mesopotamia on board of Indian, Arabian or Mesopotamian ships. Who brought the cloves until the eastern or southern terminal(s) of these ships, and where this terminal(s) was, is just matter of speculation. I don't consider cloves an evidence of Austronesian presence, because commodities may travel independently from people. The quantity of cloves so far discovered in the Middle East accounts only for an episodic trade of this commodity.?

My comment: Unless you assume that Indians, Arabians or Mesopotamians themselves sailed to Maluku, cloves cannot be but very suggestive evidence of Austronesian presence, at least up to the supposed eastern or southern terminal. Sure, cloves and other South East Asian specialties may have been rare, exotic and expensive, but that was what I myself assumed from the very beginning. The irregularity of these exchanges does not give us license to dismiss the question of

the exchange route: amber reached Bronze Age Mediterranean very seldom, but serious scholars would very much want to know how amber consignments reached their destinations, especially when it seems that an early consignment might have arrived by sea, all the way from the British Isles.

(2) My question: Given that you had read just four previous postings of the 'Austric' list, what were the posts you were referring to?

Your reply: 'Not one post in particular, but a more general idea that I felt from your correspondence with Paul K. Manasala. Actually the first post I read refers also to Eden in the East, and Oppenheimer actually follow a diffusionistic perspective.'

My comment: And you call that scholarship? I referred to Oppenheimer as I was encouraged to check his book but, alas, I already had read it, unlike the person who gave the recommendation. As it happens, the book is very much worth reading, albeit not for the reasons you inferred from my comments. Those of us who are interested in the population history of the Pacific Ocean and who have some (admittedly modest) understanding of archaeogenetics know that Oppenheimer's criticism of the (Renfrew inspired) Bellwood/Blust position is of good scientific value. Moreover, his presentation of the Solheimian Nusantara perspective is illuminating and worth considering. Taking seriously these views does not turn anybody a 'hyperdiffusionist'. Indeed, my attitude towards Oppenheimer's more speculative ideas is quite critical and I certainly reject all 'hyperdiffusionism' as naive and misguided.

(3&4&5) My questions: 'Do you think that we have no good evidence for believing that during classical antiquity Greece, Rome et al. participated in trade with 'genuine' cinnamon and cassia? Do you believe that 'cassia' and 'cinnamon' were from the Horn of Africa? And if so, could you give your views about their real nature? Do you suggest that whatever 'cinnamon' and 'cassia' might have meant, there is no

evidence in classical sources that Eastern Africa was involved in trading them?

Your replies: ?I think we have no evidence at all. We have two names of spices that according to the Greek authors came from the Horn of Africa, and cinnamon mostly from the interior, not from the coast. As I said, there were also others opinions among the Greek geographers; this is a longer argument, but in short it is possible to explain these different opinions mostly with the shifting of the trade routes through the time, and demonstrate that cassia and cinnamon ultimately came out of Africa.

Pliny (XII 89-95 (42)) has quite a long description of the cinnamon plant, in addition to which he explains how people cropped it, and describes even the religious rituals performed by the people during the harvesting season. He placed this area in Africa, and I don't see any reason to believe that he confused Africa with SE Asia. Pliny barely knew that there was something beyond India, so it is not reasonable to suppose that his sources reported something they saw in SE Asia. From his evidence we can only conclude that people harvested cinnamon in Africa; so, it was an African plant. Which one, I don't know.?

?To the first question my answer is yes. To the second: no, I cannot because nobody identified any african botanical species which might be the origin of cassia and cinnamon. I'm perfectly aware that this is the weakest point of my opinion: if there is no african plant giving something that might be cinnamon, the only reasonable explanation is an Asian import. However, Greek and Latin sources knew that cinnamon grew in Africa, not that it was just sold there. Besides this, as I said above, if cinnamon arrived in Africa through a southern oceanic route, I don't understand why cinnamon is not recorded in the marts south of today Somalia. See also above (no 4) for Pliny's evidence.?

?Yes, this is exactly what I meant. Let's say Southeastern Africa, to avoid confusion with Somalia, for instance.?



My comments: I fully agree with you that we must have sound evidence for the arrival of South East Asian spices to ancient West before we start building speculative superstructures on mere conjectures. That was the very reason why I asked Paul Manansala to give me scientifically corroborated evidence for cloves: if we can ascertain ourselves the identity of the substance, we need not start arguing about ancient texts.

As for these classical texts, I think that they leave all too much room for debate, either way. Still, I think that we may infer on their basis that (a) the South Arabians (and other traders along the Gulf of Aden) made handsome profits for selling the Greeks and the Romans something that was called 'cinnamon' and 'cassia' and that (b) South Arabians et al. wanted to protect their lucrative profits by hiding the real source of 'cinnamon' and 'cassia'. Now, it is surely possible that the South Arabians et al. might have been secretive even if the sources of 'cassia' and 'cinnamon' were in their own territory, but it is not very likely: the ancients usually made up fabulous stories when they were afraid that someone else could find his way to a source that was not firmly under their own control. Given this, it would be only to be expected if the classical authors failed to identify the real source of 'cassia' and 'cinnamon' and would make erroneous guesses about these sources. And it is at least plausible (although not compelling in any sense), as you yourself admit, that 'cinnamon' and 'cassia' might have meant what they mean for us: cinnamon and cassia, spices from the East. Indeed, any other theory owes us an equally plausible theory of the true nature of 'cassia' and 'cinnamon' and a plausible theory how the semantics of those words changed. Of course, there are authors supporting your view: Patricia Crone seems to think she has a promising candidate for 'cinnamon', coming from the Somaliland.

As classical texts and terms leave open intolerable uncertainties, it is obvious to me that we cannot hope to prove the Austronesian involvement on their basis.

We need solid independent evidence, the sort of evidence the Terqa excavations seem to provide. And I agree that your question 'if cinnamon arrived in Africa through a southern oceanic route, I don't understand why cinnamon is not recorded in the marts south of today Somalia?' is a good question: one would expect that at least one classical author would have heard rumours about South Arabians et al. sailing to Rhapta (8 degrees southern latitude) to find cinnamon and cassia. And more to the point: we should find some remains of 'genuine' cassia and cinnamon from the coast of East Africa, south of the equator.

(6) My question: Do you suggest that it is unproblematically true that the Greek and Roman sources did not refer to Austronesians when they discussed the inhabitants of Eastern Africa? If so, what might be the textual and other arguments?

Your reply: 'There is not a word in Greek and Latin sources on people arriving in Eastern Africa coming from the other side of the Indian Ocean. The only possibility is to find possible austronesian roots in the place names of Azania. Unfortunately, most of the names of coastal sites of Azania do not reproduce the local name: they are Greek names given to African places for the benefit of Roman empire subjects (Rhapta is a case in point).'

My comment: Once again, I agree with you about the necessity of having independent linguistic and archaeological evidence. And I agree that the classical texts, for reasons I have already given, cannot be used to prove the Austronesian presence. Still, I am interested to know how you would phrase in English and interpret the controversial passages from Periplus and Pliny: i.e., the chapters 15-17 from Periplus and the passage (IX, 86) from Pliny

'Non sunt eorum cinnamomum aut casia, et tamen felix appellatur Arabia, falsi et ingrati cognominis, quae hoc acceptum superis ferat, cum plus ex eo inferis debeat. beatam illam fecit hominum etiam in morte luxuria quae dis intellexerant genita inurentium defunctis.'

periti rerum adseverant non ferre tantum annuo fetu,  
quantum Nero princeps novissimo Poppaeae suae die  
concremaverit. aestimentur post ea toto orbe singulis  
annis tot funera acervatimque congesta honori  
cadaverum quae dis per singulas micas dantur. nec  
minus propitii erant mola salsa supplicantibus, immo  
vero, ut palam est, placatiores. verum Arabiae  
etiamnum felicius mare est;

ex illo namque margaritas mittit. minimaque  
computatione miliens centena milia sestertium annis  
omnibus India et Seres et paeninsula illa imperio  
nostro adimunt: tanti nobis deliciae et feminae  
constant. quota enim portio ex illis ad deos, quaeso,  
iam vel ad inferos pertinet?

Cinnamomum et casias fabulose narravit antiquitas  
princepsque Herodotus avium nidis et privatim  
phoenicis, in quo situ Liber pater educatus esset, ex  
inviis rupibus arboribusque decuti carnis quam ipsae  
inferrent pondere aut plumbatis sagittis, item casiam  
circa paludes, propugnante unguibus diro  
vespertilionum genere aligerisque serpentibus, his  
commentis augentes rerum pretia.

comitata vero fabula est ad meridiani solis  
repercussus inenarrabilem quendam universitatis  
halitum e tota paeninsula existere tot generum aerae  
spirante concentu, Magnique Alexandri classibus  
Arabiam Trogodytis conubio permixta.

hi mercatores id a conterminis vehunt per maria vasta  
ratibus, quas neque gubernacula regant neque remi  
trahant vel inpellant vela, non ratio ulla adiuvet:  
omnium instar ibi sunt homo tantum et audacia.  
praeterea hibernum mare exigunt circa brumam, euris  
tum maxime flantibus.

hi recto cursu per sinus inpellunt, atque a  
promunturii ambitu argestae deferunt in portum  
Gebbanitarum qui vocatur Ocilia. quam ob rem illi  
maxime id petunt, produntque vis quinto anno reverti  
negotiatores et multos interire. contra revehunt  
vitrea et aena, vestes, fibulas cum armillis ac  
monilibus. ergo negotiatio illa feminarum maxime fide

constat.?

(I have borrowed here the Latin version that is available at Lacus Curtius, at

[http://www.ukans.edu/history/index/europe/ancient\\_rome/L/Roman/Texts/Pliny\\_the\\_Elder/12\\*.html](http://www.ukans.edu/history/index/europe/ancient_rome/L/Roman/Texts/Pliny_the_Elder/12*.html)

)

(7) My question: Who claimed that the Kunlun were the only people able to navigate in the Indian Ocean?? And what sort of evidence would persuade you to take seriously the possibility that Austronesian mariners visited Eastern Africa, say, before 1000 BCE?

Your reply: ?I exaggerated in the answer, but I got the impression that this was what you basically supported when you assumed Solheim's Nusantara traders as the men who possibly brought the cloves all the way to Mesopotamia. As I said in my first post, and also above here, the Near Eastern people were able to sail on the Ocean at least from the III millennium BC. We have archaeological evidence of this, but no evidence of people coming from some place east of India into Middle Eastern waters.?

My comment: I do not think it is impossible that Nusantara traders (?sea gypsies?) might have sailed all the way from Maluku to Mesopotamia; it is certainly as likely as a Mesopotamian/Arabian/Indian voyage to Maluku and back India/Arabia/Mesopotamia. However, I never claimed that Nusantara traders/explorers/pirates sailed to Mesopotamia; instead I raised the possibility that they might have reached the coasts of East Africa already 2000 BCE. And a possible contact with South Arabians et al. might have taken place somewhere in the Somali coasts. Why do I then prefer Austronesians to Western sailors? Well, because Austronesians/Polynesians simply have been the best and most audacious sailors of documented human history! If you doubt my words, look at the map: the proven Austronesian/Polynesian voyages extend all the way from Madagascar to Easter Island ? quite amazing!

(8) My question: Do you think that this textual argument is non-problematic?

Your reply: ?It is a fact: the only two Greek sources mentioning Rhapta (Periplus and Ptolemy) do not record aloe or coconuts. Please, understand me, because this seems actually the origin of a lot of misunderstanding in our conversation: I don't mean they didn't exist in or near Rhapta at that time: only that the Roman time sources do not mention them (this difference is basic).?

My comment: There seems to be some uncertainty over translating the passage on Rhapta exports: some earlier translations (such as Schoff 1912) suggested that palm-oil (and hence coconuts) were exported from Rhapta, later translations may treat this differently. Once again, I agree that the textual evidence for Austronesian presence is inconclusive at most and that if we take it as face value, we have evidence mostly in favour of cinnamon/cassia being traded at the Horn of Africa, not in equatorial Africa.

(9) My question: Do you mean ?cinnamon and cassia? or ?cinnamon? and ?cassia? ? or both? And would the discovery of either make you change your views on the possibility of Austronesian dominated trade of cinnamon, cassia and cloves trade?

Your reply: ?I mean both. For the second question the answer is, obviously, yes. I probably gave the impression to fight for the sole true faith! I assure everybody that this is not the case; otherwise I wouldn't have subscribed to this mailing list. But I dislike to reconstruct ancient history basing only on suppositions. I understand that this way to proceed is much less exciting, but it is closer to the real documentation.?

My reply: I share your demand for sound evidence. And to whet your appetite for more, I present here the evidence that made me to join the ?Austrian? list in the first place:

Banana and Plantain: the Earliest Fruit Crops? by

Prof. Edmond De Langhe, Founding Director of INIBAP,  
Belgium

at

<http://www.inibap.org/publications/annualreport/focus195.pdf>

and

First archaeological evidence of banana cultivation in  
central Africa during the third millennium before  
present, Vegetation History and Archaeobotany, Table  
of Contents Vol. 10 Issue 1 by

Christophe Mbida Mindzie, Hughes Doutrelepon, Luc  
Vrydaghs, Rony L. Swennen, Rudy J. Swennen, Hans  
Beeckman, Edmond de Langhe, Pierre de Maret:

at

<http://link.springer.de/link/service/journals/00334/>

I thank you for your prompt reply and apologise for my  
lateness in responding,

Best regards,

Juha Savolainen

P.S. I will respond to Paul Kekai Manansala soon.  
Thanks for his patience as well.

---

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| 73|2002-07-30 08:26:59|a.manansala@attbi.com|Re: Comments for Oscar Nalesini|

- > Your reply: ?It is a fact: the only two Greek sources
- > mentioning Rhapta (Periplus and Ptolemy) do not record
- > aloe or coconuts. Please, understand me, because this
- > seems actually the origin of a lot of misunderstanding
- > in our conversation: I don't mean they didn't exist in
- > or near Rhapta at that time: only that the Roman time

- > sources do not mention them (this difference is
- > basic).?
- >
- > My comment: There seems to be some uncertainty over
- > translating the passage on Rhapta exports: some
- > earlier translations (such as Schoff 1912) suggested
- > that palm-oil (and hence coconuts) were exported from
- > Rhapta, later translations may treat this differently.
- > Once again, I agree that the textual evidence for
- > Austronesian presence is inconclusive at most and that
- > if we take it as face value, we have evidence mostly
- > in favour of cinnamon/cassia being traded at the Horn
- > of Africa, not in equatorial Africa.
- >
- >

The Periplus mentions five grades of cinnamon traded near Rhapta especially at the Opone market: gizir, asypha, arebo, molu and mosyllum.

Dioscorides in his botanical description of cinnamon mentions all these grades and two more: darki and qitto.

Pliny states that tarum and cancanum come from the 'the cinnamon and cassia producing land.'

These spices following Dioscorides and others have been identified as aloe wood and benzoin respectively. Both of these are SE Asian products.

I don't have a copy of the original or Schoff with me, but the latter did note a word in Periplus for "coconut."

Btw, the words for coconut in East Africa are all believed to be derived directly from Austronesian. You can check this out in the message archive at the ICCRA group.

Regards,  
Paul Kekai Manansala

| 74|2002-07-30 08:38:34|pinatubo.geo|Re: Comments for Oscar Nalesini|

--- In austric@y..., a.manansala@a... wrote:

>  
>  
>

- > The Periplus mentions five grades of cinnamon traded
- > near Rhapta especially at the Opone market: gizir, asypha,
- > arebo, molu and mosyllum.
- >

Note also from Ptolemy:

Mosylum promontory and market place 79°00' 900

This is south of the coordinates given for Rhapta by Ptolemy.  
Cosmas also places the cassia market in Azania.

Regards,  
Paul Kekai Manansala

| 75|2002-07-30 09:03:28|Juha Savolainen|Some texts|

Dear Paul,

Just to give some of the relevant facts, I reproduce  
here the textual evidence.

What follows first is part of the W.H. Schoff's (tr. & ed.), *The Periplus of the Erythraean Sea: Travel and Trade in the Indian Ocean by a Merchant of the First Century* (London, Bombay & Calcutta 1912), available in its totality at

<http://www.fordham.edu/halsall/ancient/periplus.html>

"12. Beyond this place, the coast trending toward the south, there is the Market and Cape of Spices, an abrupt promontory, at the very end of the Berber coast toward the east. The anchorage is dangerous at times from the ground-swell, because the place is exposed to the north. A sign of an approaching storm which is peculiar to the place, is that the deep water becomes more turbid and changes its color. When this happens they all run to a large promontory called Tabae, which offers safe shelter. There are imported into this



market town the things already mentioned; and there are produced in it cinnamon (and its different varieties, gizir, asypha, areho, iriagia, and moto) and frankincense.

13. Beyond Tabae, after four hundred stadia, there is the village of Pano. And then, after sailing four hundred stadia along a promontory, toward which place the current also draws you, there is another market-town called Opone, into which the same things are imported as those already mentioned, and in it the greatest quantity of cinnamon is produced, (the arebo and moto), and slaves of the better sort, which are brought to Egypt in increasing numbers; and a great quantity of tortoiseshell, better than that found elsewhere.

14. The voyage to all these farside market-towns is made from Egypt about the month of July, that is Epiphi. And ships are also customarily fitted out from the places across this sea, from Ariaca and Barygaza, bringing to these far-side market-towns the products of their own places; wheat, rice, clarified butter, sesame oil, cotton cloth, (the monache and the sagmatogene), and girdles, and honey from the reed called sacchari. Some make the voyage especially to these market-towns, and others exchange their cargoes while sailing along the coast. This country is not subject to a King, but each market-town is ruled by its separate chief.

15. Beyond Opone, the shore trending more toward the south, first there are the small and great bluffs of Azania; this coast is destitute of harbors, but there are places where ships can lie at anchor, the shore being abrupt; and this course is of six days, the direction being south-west. Then come the small and great beach for another six days' course and after that in order, the Courses of Azania, the first being called Sarapion and the next Nikon; and after that several rivers and other anchorages, one after the other, separately a rest and a run for each day, seven in all, until the Pyralax islands and what is called the channel; beyond which, a little to the south of south-west, after two courses of a day and night along the Ausanitic coast, is the island Menuthias, about

three hundred stadia from the mainland, low and wooded, in which there are rivers and many kinds of birds and the mountain-tortoise. There are no wild beasts except the crocodiles; but there they do not attack men. In this place there are sewed boats, and canoes hollowed from single logs, which they use for fishing and catching tortoise. In this island they also catch them in a peculiar way, in wicker baskets, which they fasten across the channel-opening between the breakers.

16. Two days' sail beyond, there lies the very last market-town of the continent of Azania, which is called Rhapta; which has its name from the sewed boats (rhapton ploiarion) already mentioned; in which there is ivory in great quantity, and tortoise-shell. Along this coast live men of piratical habits, very great in stature, and under separate chiefs for each place. The Mapharitic chief governs it under some ancient right that subjects it to the sovereignty of the state that is become first in Arabia. And the people of Muza now hold it under his authority, and send thither many large ships; using Arab captains and agents, who are familiar with the natives and intermarry with them, and who know the whole coast and understand the language.

17. There are imported into these markets the lances made at Muza especially for this trade, and hatchets and daggers and awls, and various kinds of glass; and at some places a little wine, and wheat, not for trade, but to serve for getting the good-will of the savages. There are exported from these places a great quantity of ivory, but inferior to that of Adulis, and rhinoceros-horn and tortoise-shell (which is in best demand after that from India), and a little palm-oil.

18. And these markets of Azania are the very last of the continent that stretches down on the right hand from Berenice; for beyond these places the unexplored ocean curves around toward the west, and running along by the regions to the south of Aethiopia and Libya and Africa, it mingles with the western sea.?

I also reproduce here, from Bill Thayer's amazing

Lacus Curtius site,

[http://www.ku.edu/history/index/europe/ancient\\_rome/E/Roman/home.html](http://www.ku.edu/history/index/europe/ancient_rome/E/Roman/home.html)

Book IV of Ptolemy's 'Geography' where Ptolemy maps some of the relevant locations (look at numbers at the right, they tell the latitude, S signals south of the equator)

Book IV, Chapter 7

Location of Aethiopia below Egypt

(from the Fourth Map of Libya)

Aethiopia, which is below Egypt, is terminated, as we have indicated, on the north by Libya and Egypt; on the west by a part of Interior Libya along the meridian extending from Darnis to the southern terminus of Libya, which is located in 51°15'30" S

on the south by the line leading from this terminus along the remaining part of the Aethiopian interior to:

the Rhaptum promontory, which is located in 73°50'25" S

It is terminated on the east by a part of the Bay of Arabia and the Red Sea, and the Barbarian Sea to the Rhaptum promontory, the description of which is the following:

After the Bazium promontory referred to above:

Prionotus mountains 65°00'22"30

Chersonesus 65°00'22"00

Mnemeum promontory 65°30'21"30

Isius mountains 65°30'21"20

a deep harbor 65°00'21"10

Dioscuror harbor 65°00'21"00

Cereris Speculae promontory 65°20'20"15

Aspis promontory 65°30'19"45

Diogenis promontory 65°40'19"40

Satyron mountains 65°40'19"00

Monodactylus mountains 65°30'18"30

Taurus mountains 65°40'18"00

Harbor Deorum Tutorum 65°30'17"30

Evangelon harbor 65°45'17"00

Ptolemais Venationum 66°00'16"25

Ara Eratonis promontory 66°30'16"00

Sabasticum mouth 67°00'15"00

a large coast 66°00 14'15  
 Colobon promontory 68°00 13'40  
 Sabat town 68°00 12'30  
 and in the Aduliticus Bay,  
 a mountainous peninsula 68°00 12'10  
 Adulis 67°00 11'40  
 Saturni promontory 68°00 11'40  
 Antiphili Portus 72°00 10'15  
 Mandaith village 73°15 10'20  
 Arsino? 73°45 10'40  
 After the strait in the Red Sea,  
 Dire town in the promontory 74°30 11'00  
 Then in Avalites Bay,  
 Avalites market place 74°00 8'25  
 Malao market place 75°00 6'30  
 Mondu market place 78°00 7'00  
 Mosylum promontory and market place 79°00 9'00  
 Cobe market place 80°00 8'00  
 Elephas mountains 81°00 7'30  
 Acanna market place 82°00 7'00  
 Aromata promontory and market place 83°00 6'00  
 In the Barbarian Bay,  
 Pano village 82°00 5'00  
 Opone market place 81°00 4'15  
 Zingis promontory 81°00 3'30  
 Phalangis mountains 80°00 3'30  
 Apocopa 70°00 3'00  
 Austri Cornu promontory 79°00 1'00  
 a small coast 78°00 1'00 S  
 a large coast 76°00 2'00 S  
 Essina 73°30 3'30 S  
 Sarapionis station and emporium 74°00 3'00 S  
 Tonice market place 73°00 4'15 S  
 mouth of the Rhaptus river 72°30 7'00 S  
 Rhapta, metropolis of Barbaria, a short distance from  
 the sea 71°00 7'00 S  
 Rhaptum promontory 73°50 8'25 S  
 The remaining part of the Nile, after the Great  
 Cataract, is described as follows thru the names of  
 the villages adjacent to it:  
 After Pselcis and the Lesser Cataract,  
 the location of which is in 60°30 22'30  
 on the west bank of the river are the villages  
 Tasitia 60°30 22'00  
 Boon 62°00 21'40  
 Autoba 61°30 21'30

Phthruai 61°15' 21"20

Pistre 61°00' 20"40

Ptemithis 61°00' 20"15

Abuncis 59°30' 20"00

Cambysis Aerarium 59°00' 18"00

Erchoas 59°30' 18"00

Satachtha 60°30' 18"00

Moru 61°30' 18"40

Nacis 62°00' 19"30

Tathis 61°00' 17"00

On the east bank of the river are the villages

Pnups 62°00' 22"00

Berethis 62°00' 21"30

Gerbo 62°00' 21"00

Pataeta 61°40' 20"30

Pontyris 61°00' 20"00

Primis Minor 60°00' 19"30

Arbis 60°30' 18"30

Napata 63°00' 20"15

Sacole 63°00' 19"30

Sandace 63°00' 18"30

Orbadaru 62°40' 18"00

Primis Major 62°00' 17"00

Here the Nile River on the west and the Astaboras on the east form the region of the island of Mero?, in which island are the following towns:

Mero? 61°30' 16"25

Sacolche 61°40' 15"15

Eser 61°40' 13"30

Daron village 62°00' 12"30

The junction of the river Nile and the river Astapus 61°00' 12"00

Then the junction of the river Astaboras and the Astapus 62°30' 11"30

Where the Nile river becomes one thru the union of rivers which flow from two lakes 60°00' 2"00

Western lake 57°00' 6"00 S

Eastern lake 65°00' 7"00 S

Lake Colo?, from which flows the Astapus river 69°00' equator

The towns remote from the river in the interior are:

Axume where the king's palace is 65°00' 11"00

Colo? town 69°00' 4"15

Maste town 65°00' 4"15 S

The mountains in this region toward the west of the Nile river, extending along the entire Nile, which are

commonly called the Aethiopian mountains, are celebrated;  
the position of which is 55°00' 23"00  
and 55°00' 8"30  
The mountains to the east of the Nile are called the Garbatum,  
the middle of which is in 69°00' 6"00  
and the Elephas mountains 78°00' 5"30  
and those near the lake are called the Pylaei  
mountains 65°00' equator  
the Maste mountains 68°00' 5"00 S

The land which is near the Arabian Bay and the Avalites gulf, along the sea is called Troglodytica as far as the Elephas mountains, in which region are the Adulitae, and the Avalitae near a bay of this name, and the Mosyli above the promontory with a market place of this name. The entire seacoast to the Rhaptum promontory is called Azania; the interior region is called Barbaria, in which there are many elephants.

The Colobi occupy that part of the region toward the east from the river which is near the Bazium promontory; next to these toward the south are the Tabieni; then the Sirtibes; next to these are the Attiri; then the Babylleni and the Rhizophagi; then the Axumitae and the Sobridae; next the Molibae, the Megabardi, and the Nubae toward the west from the Avalitae; then below the Molibae are the Blemyes; below whom are the Dedacae, and the Pechini between the river Astapus and the Garbatum mountains; from whom toward the west are the Strathophagi Aethiopes; toward the south from the mountain are the Catadrae and the Myrrhifera the land stretching up to Lake Colo?, after which are the Mastitae to the lake of the Nile.

To the west, from this part of the Nile river, those occupy the land after the Greater Cataract, who pasture the Triacontaschoenus region between the Aethiopian mountains and the Nile river, after these toward the south are the Euonymitae; then Aethiopia Media and the Sebridae; these races also inhabit the island of Mero?, and below them are the Gapachi; below these the Ptoemphanae, and below these the Cadupi; next to these are the Elephantophagi Aethiopes; below

these the Pesendarae, and beyond the lake the Cinnamon-bearing land; moreover between the Nile and the Astapus river, toward the island of Mero?, are the Memnones and more to the south are the Sapaei. In the remaining parts of the land toward the west from the Aethiopian mountains next to the sandy and dry region dwell the Phazaniae and the Bacalitides races; then the Scenitae and the Tralletae, after these the race of the Daradi; then the Orypaei Venatores next to these the Nygbenitae Aethiopians.

The following islands are near Aethiopia below Egypt in the Arabian Bay:

Astarta island 66°00' 22'30"  
Ara Minervae island 66°10' 21'30"  
Gypsitis island 67°00' 19'40"  
two islands of Gomadean 67°30' 19'00"  
Myronis island 67°00' 18'00"  
Catathra or Chelonitides islands, two in number 68°00' 17'30"  
Orisitides, two islands 67°30' 17'00"  
Magorum island 68°00' 16'00"  
Daphnine island 68°30' 15'20"  
Acanthine island 68°30' 15'00"  
Macaria island 68°30' 14'00"  
Avium island 69°00' 14'00"  
the Bacchi and Antibacchi 69°30' 13'15"  
Panis island 68°40' 12'00"  
Diodori island 70°00' 12'30"  
Isidis island 70°00' 11'30"  
In the Bay of Avalites is  
Mondi island 77°00' 8'30"  
Next to Aromata are these islands:  
Amici island 85°00' 4'00"  
the two Menae islands 84°00' 2'30"  
Myrice island 83°30' 1'00"

Then to the east of these islands is the sea called Hippalum near which is the Indian Sea.

Best regards, Juha Savolainen

---

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| 76|2002-07-30 09:04:03|a.manansala@attbi.com|Re: Comments for Oscar Nalesini|

Sorry, I have made an error in my last few posts.

It should be south of Adulis (11°40' N, also Axum 11° N)  
and not Rhapta.

The Opone marketplace is placed in the "Barbarian Bay"  
along with Rhapta, although it is further north.

Regards,  
Paul Kekai Manansala

> --- In austri@y..., a.manansala@a... wrote:

>>

>>

>>

>

>> The Periplus mentions five grades of cinnamon traded  
>> near Rhapta especially at the Opone market: gizir, asypha,  
>> arebo, molu and mosyllum.

>>

>

> Note also from Ptolemy:

>

> Mosyllum promontory and market place 79°00' 9'00"

>

> This is south of the coordinates given for Rhapta by Ptolemy.

> Cosmas also places the cassia market in Azania.

>

> Regards,

> Paul Kekai Manansala

>

>

>

>

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| 77|2002-07-30 11:23:07|Juha Savolainen|Comments for Paul Kekai Manansala|

Comments for Paul Kekai Manansala:

Dear Paul,

It was good that you finally realised your error: you have confused Ptolemy's longitudes with latitudes?

I hope that you have enough stamina to take the next and absolutely necessary step: to recognise that your confusion invalidates nearly everything that you wrote in the citations that now follow:

?Pliny the Elder, Theophrastus, Strabo, Pomponius Mela, Ptolemy and many other authors all the way to the time of Cosmas Indicopleustes mention the spice trade including cassia and cinnamon either in Rhapta or in the area around or south of Azania known as Regio Cinnamomifera.?

?Whatever the date of the Austronesian arrival, in medieval times, cinnamon landing in Madagascar would then find its way to Rhapta in present-day Somalia. From there it reached the South Arabian traders in Muza, Yemen who carried on the trade along the coasts of the Red Sea.?

?Actually the Regio cinnamomifera? is widely placed on the SE coast of Africa by modern scholars based on works like Strabo, Cosmas, etc. They clearly place the location of the cinnamon market in or south of Azania, not in Arabia.?

?The strongest evidence in Greek and Latin texts is that cinnamon and cassia come from the SE coast of Africa.?

??Ptolemy clearly locates Azania to the south in the region of Rhapta. The Regio Cinnamomifera is universally placed either in Azania or south of Azania."

?Ptolemy states quite clearly that these ports were markets for spices and aromatics. From other sources including Pliny it is clear that these spices included cinnamon and cassia that were traded further north eventually reaching Europe.?

?The Periplus mentions five grades of cinnamon traded near Rhapta especially at the Opone market: gizir, asypha, arebo, molu and mosyllum.?

?I don't have a copy of the original or Schoff with me, but the latter did note a word in Periplus for "coconut."

?Note also from Ptolemy:

Mosylum promontory and market place 79°00' 9"00'

This is south of the coordinates given for Rhapta by Ptolemy. Cosmas also places the cassia market in Azania.?

Having said this, I suggest that (a) you apologise Oscar who has trying to tell you the obvious and (b) do not lose your heart as you have otherwise made a great service for all of us for putting together pieces that are truly interesting and deserve a very careful study. We all make errors, the point is to learn from them.

Best regards, Juha Savolainen

---

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| 78|2002-07-30 12:16:16|a.manansala@attbi.com|Re: Comments for Paul Kekai Manansala|

> Comments for Paul Kekai Manansala:

>

> Dear Paul,

>

> It was good that you finally realised your error: you

- > have confused Ptolemy's longitudes with latitudes?
- >
- > I hope that you have enough stamina to take the next
- > and absolutely necessary step: to recognise that your
- > confusion invalidates nearly everything that you wrote
- > in the citations that now follow:

Not really. Let's examine each statement:

- >
- > ?Pliny the Elder, Theophrastus, Strabo, Pomponius
- > Mela, Ptolemy and many other authors all the way to
- > the time of Cosmas Indicopleustes mention the spice
- > trade including cassia and cinnamon either in Rhapta
- > or in the area around or south of Azania known as
- > Regio Cinnamomifera.?
- >

These authors do recognize the cinnamon and cassia trade in the general region of Azania, Rhapta or the Regio cinnamomifera.

The location of Azania, of course, differs but the most common description has it starting at Cape Guardafui (modern Ras asir) and incorporating all the coastal areas southward.

Periplus, for example, mentions the cinnamon trade at Opone, which under this description would include Azania.

Ptolemy states that Opone was in the Barbarian Bay and Barbaria is described as the region inland of Azania.

In terms of the Regio Cinnamomifera, of course, there had to be trade from there. That's where the cinnamon and cassia came from in the first place. The question is where did the ancients think this region was?

Generally it was located far to the south. Ptolemy says it is beyond the Nile Lakes which he places

at 7 or 8 degrees south.

- > ?Whatever the date of the Austronesian arrival, in
- > medieval times, cinnamon landing in Madagascar would
- > then find its way to Rhapta in present-day Somalia.
- > From there it reached the South Arabian traders in
- > Muza, Yemen who carried on the trade along the coasts
- > of the Red Sea.?
- >

This is, of course, following Miller except Rhapta would be in Tanzania. I agree with this theory.

- > ?Actually the Regio cinnamomifera? is widely placed on
- > the SE coast of Africa by modern scholars based on
- > works like Strabo, Cosmas, etc. They clearly
- > place the location of the cinnamon market in or south
- > of Azania, not in Arabia.?
- >

Again, this is correct. The reconstructed maps I gave showed that the Regio Cinnamomifera was located in the extreme south of Africa, same as Azania. This included the coastal regions and it would agree with the descriptions given by Pliny of the sea voyage starting in the winter.

Notice on both maps that the Regio is located "beyond" the Nile Lakes (west and east) which agrees with Ptolemy.

Both Strabo and Eratosthenes recognized the Regio Cinnamomifera as the southernmost inhabitable land in Africa. The same is said of Azania in other texts.

Cosmas also states explicitly that cassia was traded in Azania.

The error that I made was in mixing up Adulis, the

northern port before reaching Muza with Rhapta. I apologize for that as I've been very busy and posting as I'm doing other things at the same time.

Regards,  
Paul Kekai Manansala

| 79|2002-08-13 14:03:08|a.manansala@attbi.com|Downloadable books (\_Bontoc Igorots\_ and \_Negritos of Zambales\_)|

From: Fr?deric Plessis  
Date: Mon Jul 15, 2002 2:00 am  
Subject: The Bontoc Igorots (Jenks 1905) for free !

<<http://64.156.188.97/yh1r/yh1r6eg.gif?1029272430>>

Dear Colleagues,  
I'm forwarding this from the Austronesian list.

Regards,  
Paul Kekai Manansala

---

--- In austronesian@y..., Fr?deric Plessis wrote:

> Dear Colleagues,  
>  
> Thanks to Bohol.ph, several classics of Philippine

ethnography are now

> available on the internet. The full-text version of

The Bontoc Igorots

> (Jenks 1905) and the Negritos of Zambales (Reed 1905)

can be downloaded at

- > <http://www.bohol.ph>
- > Other publications also available on this website

include 4 articles on

- > philippine folktales.
- >
- > F.Plessis
- > Austronesian Network

| 80|2002-08-20 19:45:17|a.manansala@attbi.com|ABSTRACT: JC virus genotypes in the western Pacific suggest Asian |

Hum Biol 2002 Jun;74(3):473-88

JC virus genotypes in the western Pacific suggest Asian mainland relationships and virus association with early population movements.

Yanagihara R, Nerurkar VR, Scheirich I, Agostini HT, Mgone CS, Cui X, Jobes DV, Cubitt CL, Ryschkewitsch CF, Hrady DB, Friedlaender JS, Stoner GL.

Retrovirology Research Laboratory, Pacific Biomedical Research Center, University of Hawaii at Manoa, Honolulu 96822, USA.

Distinct genotypes of human polyomavirus JC (JCV) have remained population associated possibly from the time of dispersal of modern humans from Africa. Seven major genotypes with additional subtypes serve as plausible markers for following early and more recent human migrations in all parts of the world. Phylogenetic trees of JCV sequences from the major continental population groups show a trifurcation at the base indicating early division into European, African, and Asian branches. Here, we have explored JCV relationships in the island populations of the western Pacific. Since these islands were settled from the Asian mainland and islands of Southeast Asia, we expected that their virus genotypes might show an Asian connection. We found that Type 2E (Austronesian) and Type 8 (non-Austronesian) are widely distributed in western Pacific populations. A few south China strains were found (Type 7A). A subtype of Type 8, Type 8A, was

confined to Papua New Guinea. In keeping with these assignments we find that phylogenetic analysis by neighbor-joining and maximum parsimony methods places Type 2E in a closer relationship to east Asian mainland strains such as Type 2A and Type 7. Our findings support the Asian origins of the western Pacific JCV strains, and suggest three broad movements: an ancient one characterized by Type 8A, and then Type 8B, followed much later by migrations carrying Type 2E, which may correlate with the arrival of Austronesian-language speakers, the bearers of the "Lapita" cultural complex (approximately 3,500 to 5,000 years ago), and relatively recent movements carrying largely Type 7A (south China) strains directly from the West.

| 81|2002-08-20 19:55:02|a.manansala@attbi.com|ABSTRACT: Defining the allelic variants of HLA A19 in the western |

Hum Immunol 2002 Sep;63(9):779

Defining the allelic variants of HLA A19 in the western Indian population.

Shankarkumar U, Ghosh K, Mohanty D.

HLA Department, Institute of Immunohaematology (ICMR), Parel, Mumbai, Maharashtra, India

The population of western India is described as Australoid or proto-Australoid (elements) with Indo-Aryan racial admixture. The present study was undertaken to investigate the genetic diversity of human leukocyte antigen (HLA A19) in Western Indians, and to determine the frequency distribution of its molecular subtypes at the population level. The study revealed a high occurrence of A\*3303 (56%) in this population along with other common oriental alleles. A\*33 has been commonly observed in Asian Indians (18.1%), Hanza-Burush (15.7%), Punjabis (13.9%), and Japanese (11.2%) populations. A\*33 has been reported with low frequencies among the Australasians, East European (Czech), North African (Noba), and Eastern Europeans (Slovenian). Significantly we observed a low frequency of A\*29 and A\*74 when compared with other populations among the A19 repertoire. Prevalence of HLA A\*3303 at very high frequencies among Western Indians

may be a consequence of the founder effect, racial admixture, or selection pressure due to environmental factors among this population.

| 82|2002-08-22 22:21:09|chingdude56|ABSTRACT: mtDNA haplogroup F in a European population|

1: Eur J Hum Genet 2001 Sep;9(9):717-23

The evidence of mtDNA haplogroup F in a European population and its ethnohistoric implications.

Tolk HV, Barac L, Pericic M, Klaric IM, Janicijevic B, Campbell H, Rudan I, Kivisild T, Villems R, Rudan P.

Department of Evolutionary Biology, Institute for Molecular and Cell Biology, University of Tartu, Riia 23, 51010 Tartu, Estonia.

Mitochondrial DNA polymorphism was analysed in a sample of 108 Croatians from the Adriatic Island isolate of Hvar. Besides typically European varieties of human maternal lineages, haplogroup F was found in a considerable frequency (8.3%). This haplogroup is most frequent in southeast Asia but has not been reported before in Europe. The genealogical analysis of haplogroup F cases from Hvar suggested founder effect. Subsequent field work was undertaken to sample and analyse 336 persons from three neighbouring islands (Brac, Korcula and Krk) and 379 more persons from all Croatian mainland counties and to determine if haplogroup F is present in the general population. Only one more case was found in one of the mainland cities, with no known ancestors from Hvar Island. The first published phylogenetic analysis of haplogroup F worldwide is presented, applying the median network method, suggesting several scenarios how this maternal lineage may have been added to the Croatian mtDNA pool.

PMID: 11571562 [PubMed - indexed for MEDLINE]

| 83|2002-08-23 16:56:48|TTT UUU|Re: South America & Polynesia|

Sweet Potatocrossing the Pacific. See <http://www.science-frontier.comsf009/sf009p02.htm>

There are also notes on peanut getting to China from S. America long ago.

Some researchers also say maize or corn also made it to India and China before Columbus? Interested in more details?

Robin Day Botanist



**PlayaVerde@aol.com** wrote:

Hello, I am looking for information supporting Polynesian ancestry in South American indigenous peoples. Does anybody know where I could find it? Genetic evidence would be best, but I would also appreciate archeological, linguistic, cultural, or any other. Thanks.

Regards, Eduardo

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| 84|2002-08-24 05:54:00|harrieshc|Re: South America & Polynesia|

Robin

Please check the URL. I can't access it.

Hugh

--- In austriac@y..., TTT UUU wrote:

>

> Sweet Potato crossing the Pacific. See

<http://www.science-frontier.comsf009/sf009p02.htm>

> There are also notes on peanut getting to China from S. America long ago.

> Some researchers also say maize or corn also made it to India and China before Columbus? Interested in more details?

> Robin Day Botanist

> PlayaVerde@a... wrote: Hello, I am looking for information supporting Polynesian ancestry in South American indigenous peoples. Does anybody know where I could find it? Genetic evidence would be best, but I would also appreciate archeological, linguistic, cultural, or any other. Thanks.

>

> Regards, Eduardo

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| 85|2002-08-24 08:59:52|TTT UUU|Re: South America & Polynesia|

Ok I will check tomorrow when i am in better condiion  
but there is a lot of this material if you just search  
sweet potato dispaersal in MSN.com

What aspecs interest ou the most?

--- harrieshc <[jane@cablered.net.mx](mailto:jane@cablered.net.mx)> wrote:

> Robin  
>  
> Please check the URL. I can't access it.  
>  
> Hugh  
> --- In austric@y..., TTT UUU wrote:  
> >  
> > Sweet Potato crossing the Pacific. See  
> <http://www.science-frontier.comsf009/sf009p02.htm>  
> > There are also notes on peanut getting to China  
> from S. America  
> long ago.  
> > Some researchers also say maize or corn also made  
> it to India and  
> China before Columbus? Interested in more details?  
> > Robin Day Botanist  
> > PlayaVerde@a... wrote: Hello, I am looking for  
> information  
> supporting Polynesian ancestry in South American  
> indigenous peoples.  
> Does anybody know where I could find it? Genetic  
> evidence would be  
> best, but I would also appreciate archeological,  
> linguistic, cultural,

> or any other. Thanks.  
> >  
> > Regards, Eduardo  
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| 86|2002-08-24 22:17:54|TTT UUU|Re: South America|

Late reply.

The site I found via MSN said sweet potatoes were already present across the south pacific (Easter island, New Zealand etc.) when the Europeans entered it. However, the Spanish introduced them to the Philipines..and prob. the Portuguese or Spanish

brought them to Japan then south Korea.

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

---

>

> Sweet Potatoes come from S. America. That is the

home. In New Zealand they are

> called cu-mer-ah, in Korea they arrived from

south Japan and are called

> go-gu-ma. The best guess is that the Polynesians

moved them across the Pacific

> after obtaining them from Peru, Chile, Equador or

Columbia. They do not grow

> easily in cold climates.

Yes there's no doubt that sweet potatoes originated in

South America. Were sweet potatoes found in  
Japan

and Korea before the European discovery of the

"New World?"

Regards,

Paul Kekai Manansala

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| 87|2002-08-25 09:11:15|a.manansala@attbi.com|Re: South America|

Pigafetta mentions sweet potatoes on the Ladrones (Guam) during Magellan's voyage. If I remember right sweet potatoes are mentioned in the Philippines during Legaspi's invasion.

There is an argument for the pre-Hispanic introduction of sweet potatoes in the Philippines. The Chamorro language of Guam is closely related to Philippine languages. There are many native names for the sweet potato among the indigenous peoples. The Spanish and Americans found them growing wild even in very isolated mountain areas.

Regards,  
Paul Kekai Manansala

> Late reply.  
> The site I found via MSN said sweet potatoes were  
> already present across the south pacific (Easter  
> island, New Zealand etc.) when the Europeans entered  
> it. However, the Spanish introduced them to the  
> Philippines..and prob. the Portuguese or Spanish  
> brought them to Japan then south Korea.  
> --- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

>  
>

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>

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> >

> > Sweet Potatoes come from S. America. That is the  
> home. In New Zealand they are

> > called cu-mer-ah, in Korea they arrived from  
> south Japan and are called

> > go-gu-ma. The best guess is that the Polynesians  
> moved them across the Pacific

> > after obtaining them from Peru, Chile, Equador or  
> Columbia. They do not grow

> > easily in cold climates.

>

>

> Yes there's no doubt that sweet potatoes originated in  
>

> South America. Were sweet potatoes found in  
> Japan

> and Korea before the European discovery of the

> "New World?"

>

> Regards,

> Paul Kekai Manansala

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| 88|2002-08-25 17:25:30|TTT UUU|Re: South America|

I imagine you are correct that sweet potatoes were already in the Phillipines. I found a website on economic botany that says the Spanish were responsible for the dispersal all across the Pacific..I don't credit that with much scholarship. Will look for it again and post the web address.

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

> Pigafetta mentions sweet potatoes on the Ladrones  
> (Guam) during Magellan's voyage. If I remeber right  
> sweet potatoes are mentioned in the Philippines  
> during  
> Legaspi's invasion.

>

> There is an argument for the pre-Hispanic  
> introduction  
> of sweet potatoes in the Philippines. The Chamorro  
> language of Guam is closely related to Philippine  
> languages. There are many native names for the sweet  
>  
> potato among the indigenous peoples. The Spanish and

>

> Americans found them growing wild even in very  
> isolated  
> mountain areas.  
>  
> Regards,  
> Paul Kekai Manansala  
>> Late reply.  
>> The site I found via MSN said sweet potatoes were  
>> already present across the south pacific (Easter  
>> island, New Zealand etc.) when the Europeans  
> entered  
>> it. However, the Spanish introduced them to the  
>> Philipines..and prob. the Portuguese or Spanish  
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> > >

> > > Sweet Potatoes come from S. America. That is  
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> > > called cu-mer-ah, in Korea they arrived from  
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> > Yes there's no doubt that sweet potatoes  
> originated in

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> > South America. Were sweet potatoes found in  
> > Japan

> > and Korea before the European discovery of the  
>

> > "New World?"

> >

> > Regards,

> > Paul Kekai Manansala

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| 89|2002-08-25 17:49:51|TTT UUU|Re: South America|  
Here is the containing sweet potato notes. Claims the  
Spanish and Portuguese moved them to the Pacific.  
webpage: Dissemination of Crops:  
[Lecture notes]  
<http://agronomy.ucdavis.edu/gepts/pb143/lec12/pb1143112.htm>

Robin Day Botanist

--- TTT UUU <[gymnogoy@yahoo.ca](mailto:gymnogoy@yahoo.ca)> wrote:  
> Late reply.  
> The site I found via MSN said sweet potatoes were  
> already present across the south pacific (Easter  
> island, New Zealand etc.) when the Europeans entered  
> it. However, the Spanish introduced them to the  
> Philipines..and prob. the Portuguese or Spanish  
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> Yes there's no doubt that sweet potatoes originated  
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> South America. Were sweet potatoes found in  
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> and Korea before the European discovery of the

> "New World?"

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> Regards,

> Paul Kekai Manansala

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| 90|2002-08-25 18:04:39|TTT UUU|Re: South America & Polynesia|  
Hugh: Try this

[www.knowledge.co.uk/frontiers/sf009/sf009p02.htm](http://www.knowledge.co.uk/frontiers/sf009/sf009p02.htm)

--- harrieshc <[jane@cablered.net.mx](mailto:jane@cablered.net.mx)> wrote:

> Robin  
>



> Please check the URL. I can't access it.  
>  
> Hugh  
> --- In austriac@y..., TTT UUU wrote:  
>  
>  
>> Sweet Potato crossing the Pacific. See  
> <http://www.science-frontier.comsf009/sf009p02.htm>  
>> There are also notes on peanut getting to China  
> from S. America  
> long ago.  
>> Some researchers also say maize or corn also made  
> it to India and  
> China before Columbus? Interested in more details?  
>> Robin Day Botanist  
>> PlayaVerde@a... wrote: Hello, I am looking for  
> information  
> supporting Polynesian ancestry in South American  
> indigenous peoples.  
> Does anybody know where I could find it? Genetic  
> evidence would be  
> best, but I would also appreciate archeological,  
> linguistic, cultural,  
> or any other. Thanks.  
>  
>  
>> Regards, Eduardo  
>> Yahoo! Groups Sponsor Click here to find your  
> contact lenses!  
>> To unsubscribe from this group, send an email to:  
>> austriac-unsubscribe@y...  
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| 91|2002-08-25 18:31:04|TTT UUU|Re: South America|  
What are the dates of the ship landings for Magellan  
and Pigafetta?

Robin Day botanist

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

> Pigafetta mentions sweet potatoes on the Ladrone  
> (Guam) during Magellan's voyage. If I remeber right  
> sweet potatoes are mentioned in the Philippines  
> during  
> Legaspi's invasion.  
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> There is an argument for the pre-Hispanic  
> introduction  
> of sweet potatoes in the Philippines. The Chamorro  
> language of Guam is closely related to Philippine  
> languages. There are many native names for the sweet  
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> potato among the indigenous peoples. The Spanish and  
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> Americans found them growing wild even in very  
> isolated  
> mountain areas.  
>  
> Regards,  
> Paul Kekai Manansala  
>> Late reply.  
>> The site I found via MSN said sweet potatoes were  
>> already present across the south pacific (Easter  
>> island, New Zealand etc.) when the Europeans  
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>> it. However, the Spanish introduced them to the  
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>>> Sweet Potatoes come from S. America. That is  
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> Polynesians  
>> moved them across the Pacific

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>> Columbia. They do not grow

>>> easily in cold climates.

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>> South America. Were sweet potatoes found in  
>> Japan

>> and Korea before the European discovery of the  
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>> "New World?"

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>> Regards,

>> Paul Kekai Manansala

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| 92|2002-08-25 21:20:54|a.manansala@attbi.com|Re: South America|  
The year was 1521.

Regards,  
Paul Kekai Manansala  
> What are the dates of the ship landings for Magellan  
> and Pigafetta?  
>  
> Robin Day botanist  
> --- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:  
>> Pigafetta mentions sweet potatoes on the Ladrões  
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>> sweet potatoes are mentioned in the Philippines  
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| 93|2002-08-26 15:54:52|TTT UUU|Re: South America|

Fill me in on the history of the region. Was Magellan

the first European to land in the Phillipines? I

remember he was killed or died of dysentery (can't

remember the details) in Indonesia. Was Pigafetta the

first European to land in Guam or was he a writer who

was not present? If the Spanish had their botany

correct, if they knew the sweet potato correctly (and

not some other yam or tuber) then that may clinch it.

--- [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

> The year was 1521.

>

> Regards,

> Paul Kekai Manansala

> > What are the dates of the ship landings for

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| 94|2002-08-27 08:47:39|a.manansala@attbi.com|Fwd: Archaeology find redefines Fijian history of first peoples|

<http://commcenter.attbi.com/wmc/v/wm?cmd=List&sid=c0>

Tue, Aug 27 2002 12:34 AM AEST

Archaeology find redefines Fijian history of first peoples

The discovery of a skeleton on a Fijian island has fuelled speculation that the first people to inhabit the archipelago arrived 3,000 years ago, 500 years earlier than previously thought.

Prominent South Pacific islands' geoscientist William Dickinson, from the University of Arizona, described the find as "the most important scientific discovery of its kind in Fiji for the past 30 years".

Discovered by 15 University of the South Pacific geography students at Moturiki Island, the two-metre skeleton is believed to be of Solomon Islands origin.

Samples from the skeleton will be sent to New Zealand for radiocarbon dating, with results that could prove the estimated age of the male as 3,000 years are expected by Christmas.

"This discovery is of fundamental importance because it informs Pacific islands people of their true history... where they came from, when this happened and who else in the region they are related to," excavation project leader, Professor Patrick Nunn, said.

Geography student find

First-year Solomon Islands geography student, Chris Suri, stumbled on the skeleton beneath 60 centimetres of undisturbed sand and slit clay.

"I was very excited and finding something is very good, I was at the right place at the right time, that's all," Mr Suri said.

Mr Suri named the skeleton "Mana", meaning "the truth"



in his native dialect.

The students also found stone tools, shellfish and pottery shards featuring some of the most intricate designs typical of the Lapita people [the first settlers of the Pacific], around Mana.

"We believe that it represents a burial of Lapita age, between 1,000 BC and 800 BC we estimate," Professor Nunn said.

"If this is correct than it will be only the second Lapita-age skeleton ever discovered in the Pacific Islands," he said.

### Lapita site find

The first Lapita site discovery occurred at Natanuku Village in Ra decades ago and established the Lapita people, natural seafarers sailing across uncharted waters in this region for thousands of years, reached the Fiji islands around 2,500 years ago.

From the surroundings and manufacturing stone slabs found at the Mana site, Professor Nunn said the belief was that his people lived at the site for about 400 years surviving on the bountiful untouched lagoon filled with a wide variety of seafood.

"Mana was buried east-west, his head lies in the west, his feet in the east - this is a common burial practice for ancient skeletons in Papua New Guinea and Solomon Islands," Professor Nunn said.

"His head was raised, resting on the upper part of the torso.

"At first we thought that the head had been detached from the body and placed there.

"Now we think that, like for many ancient burials in the Eastern Solomon Islands, Mana was buried wearing an elaborate headdress which is why his head could not lie flat with the rest of his body," he said.

Professor Nunn estimates the Lapita people who originally lived at Naitabale numbered around 20 to 30 people, which gradually increased to around 50 to 80; their homes between 80 to 100 metres apart.

The occupation of Naitabale by the group ended about 2,000 years ago.

### Pottery designs

Their descendants continued to make pottery but the designs died out.

Designs on pottery shards mostly depicted faces indicating a probable widespread face-tattooing culture that were transferred to pots as reminders to relatives after death.

"Most designs are parts of faces, it is likely that these faces represented ancestors of the people who made them and that the pots decorated in this way became part of a quasi-religious cult," Professor Nunn said.

"In support of this, we noted that none of the intricately-decorated pottery we found was blackened as it would have been were it used for cooking... intricately-decorated pots were for ceremonial or cultural purposes.

"At the moment, we tentatively conclude that the oldest Lapita pottery found at Naitabale was probably imported from the Santa Cruz-Reef Islands area of eastern Solomon Islands about 1,250-1,000 BC.

"This makes it the oldest Lapita pottery found in Fiji, marking the first footprints in these islands," he said.

### Print Email

| 95|2002-08-28 05:31:56|Harries|Re: Archaeology find redefines Fijian history of first peoples| Paul

Thank you for the information about the archaeological find on Fiji at <http://abc.net.au/news/newsitems/s658052.htm>

I am copying this reply to People & Coconut group in the hope that Tevita Kete can make some on-the-spot enquiries in Fiji.

Because, from my point of view, the fact that the fossil is estimated to be 500 years older than previous datings is less significant than the location it was found - Moturiki Island. The article says "Professor Nunn said the belief was that his people lived at the site for about 400 years surviving on the bountiful untouched lagoon filled with a wide variety of seafood"

And (need it be said?) fringed by coconuts.

So did the recent find expose any 3,000 year old coconut shells? And if so are they bigger, smaller, the same as or in any way different from the coconuts that grow on Moturiki today?

Perhaps we can contact some of the archaeologists that were involved?

Hugh

>Archaeology find redefines Fijian history of first  
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>

>

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>

| 96|2002-09-08 02:47:49|kalyan97|Me-lah-ha|  
<http://www.hindunet.org/saraswati/sindhu1.pdf>

Mleccha, Milakkha or Me-lah-ha people and their language

Abstract and a key to decode epigraphs

The argument is that mleccha connoted the parole of a proto-Indo-Aryan language (or, proto-Bha\_rati\_ya language) and that mleccha speakers were the dominant population ? Bha\_ratam janam --, from the days of the R.gveda, variously referred to as da\_sa, dasyu, vra\_tya or asura ? all terms used as behavioral traits as were the terms, a\_rya or deva, connotations of 'excellence' or 'righteous behaviour'. There is not an iota of evidence, in early periods of the civilization of Bha\_ratavars.a, to treat these terms as ethnic identities.

Art, cultural texts, epigraphs, archaeological discoveries, notes on early shell- and metallurgical- techniques and economic texts are used to substantiate this argument.

A surprise result emerges: a key to decode epigraphs using rebus method and proto-phonetic variants (substratum) of present-day languages of the Sarasvati Civilization area, relating epigraphs to the professions of lapidaries and smiths: shell-/stone-/mineral-/metal-workers.

The indigenously evolved civilization matured in an extensive area from Ropar to Lothal along the banks of River Sarasvati and the cultural heritage lives on in Bha\_rata, thanks to the contributions made by mleccha (me-lah-ha), contributions exemplified by samudra manthanam (churning of the ocean), as a co-operative endeavour among bha\_ratam janam.

| 97|2002-09-08 13:43:36|Juha Savolainen|Cautionary Brief On the Mighty Saraswati |  
A Cautionary Brief on the Legendary Mighty Saraswati  
River

Devotees of the Mighty Saraswati legend, such as Dr.  
Kalyanaraman, tend to believe that this ?Saraswati?  
river disappeared nearly four thousand years ago,

presumably because of neotectonic events. Given the role Vedas give to the Saraswati river, it is no wonder Hindutvavadi - mindful of its indigenist potential - attach great value to attempts to discover the traces of this legendary river. During the last two decades, an increasing number of scientists, scholars and other enthusiasts have joined the search. Indeed, there is no shortage of claims suggesting that irrefutable evidence has been found and the existence of the Mighty Saraswati has been conclusively proven. Alas, the evidence is inadequate to establish the claims.

To cut a lengthy story short, I will give here a streamlined and condensed version of the reasoning that is often used to defend the existence of 'the Mighty Saraswati river.' It does not matter much that my version is a kind of a caricature: I want to point out here the problematic steps of the reasoning, careful exegeses of the views of particular Saraswati-enthusiasts can wait for later occasions.

So, here is a simple and a transparent argument ready for critical analysis:

1. Any river can be identical with the legendary Mighty Saraswati river if and only if that river was (a) a perennial river that flowed from the Himalayas to the Arabian Sea in an area that is limited from the west by the perennial Indus river and from the east by the perennial Ganges river and if (b) the Vedic Aryan civilization flourished along the banks of that river. (Premise)

2. (a) A perennial river flowed from the Himalayas to the Arabian Sea through the present-day Ghaggar-Hakra paleochannels and (b) the majority of the Harappan cities and sites were situated along the banks of that river. (Premise)

There is a temptation to infer from these premises a conclusion:

3. There existed a river, flowing through the present-day Ghaggar-Hakra paleochannels, that was identical with the legendary Mighty Saraswati river.

(from 1 & 2)

But that is not all that is desired. Once one adds the further premise

4. No perennial river has flowed from the Himalayas to the Arabian Sea in an area that is limited from the west by the perennial Indus river and from the east by the perennial Ganges river since the decline of the Harappan civilization. (Premise)

one can easily get:

5. The legendary Mighty Saraswati river, flowing through the present-day Ghaggar-Hakra paleochannels, ceased to exist after the decline of the Harappan civilization. (from 3 & 4)

For the Saraswati devotees, the attraction of such reasoning is obvious: as a consequence, the Saraswati river does not just become the cradle of Hindu culture but it simultaneously seems to offer a most welcome way to date the sacred Vedas to a distant past, thus circumventing the embarrassment of non-Aryan Indus Civilization preceding the immigrating Vedic Aryans and giving seemingly compelling grounds for indigenist beliefs.

But is the argument sound? A sound argument has only true premises and they are connected to the claimed conclusion only through valid inferences. Let us analyse the argument, in the version I have just given.

Premise 1: This premise is interpretative and hence open to dispute. However, it would be absurd to think that any river whatsoever could carry the exciting message: some particular identity must be provided for the legendary river. The presumably perennial nature of the river and its presumed role as the cradle of the Vedic Aryan civilization seem to be minimum conditions. Moreover, it is necessary for the indigenist case that the legendary Mighty Saraswati river flowed within the confines of the Greater Indus Valley, not in Afghanistan or in somewhere else.



Premise 2: There is dispute over both subpremises. However, for the sake of the argument, I will concede 2(b). But is 2(a) true? Well, true or not, it is certainly not obvious, not even among Saraswati-minded scientists and geologists. To see the point, I ask my readers to think what A.B. Roy and S.R. Jakhar write in 'Late Quaternary drainage disorganization, and migration and extinction of the Vedic Saraswati?' (REVIEW ARTICLES, CURRENT SCIENCE, VOL. 81, NO. 9, 10 NOVEMBER 2001 1193) :

Credit goes to Oldham<sup>33</sup> for focusing our attention to the Saraswati River, the lost river of the Vedas. He was the first to prepare a drainage map of a region (Figure 5), which presumably was drained by the Saraswati River system. Many other earth scientists<sup>19,34</sup> and scholars firmly believe that the river course now represented by the dry beds of Ghaggar and its southern counterparts, Hakra and Nara basins, constitutes the channel of the Saraswati River referred to in the Vedic and Puranic literatures. This is notwithstanding the possibility that the Ghaggar? Hakra? Nara channel could even be the left-out channel of the Sutlej. The idea that the Ghaggar? Hakra? Nara course represents the Vedic Saraswati channel, although attracted attention of a large number of workers, is not in conformity with the fact that the mythical river has been conceived as the greatest river of ancient India. In the Rig Vedic hymns the river has been referred to as naditam? saraswati, meaning the 'best of rivers' (Rv. 7.95.2), which surpasses 'in majesty and might all other river' (Rv. 7.95.2) and 'swifter than other rapid streams'. 'It comes onward with tempestuous roar (Rv. 6.61.8) bursting ridges and hills with its strong waves (Rv. 6.61.2). These descriptions of Saraswati do not appear compatible in any way with any of the dry ephemeral beds of Ghaggar, Hakra and Nara. Even if we assume that the mighty Himalayan river flowed through Rajasthan only during the Middle and Late Holocene times, and was later shifted westward or northwestward, the flow of the river must have been reduced drastically when its course merged with those now followed by the Ghaggar, Hakra and Nara. None of the tributaries of the present-day Ghaggar River system has any headwater

source connection with the Himalayan glaciers, and survives only on monsoon rains.

The Ghaggar, according to Rajaguru and Badam<sup>35</sup>, was never a mighty river during the Harappan times. There is also no proof to suggest that Ghaggar ever had a flow pattern matching with that of the Vedic Saraswati. The present width of the Ghaggar has been overemphasized<sup>36</sup>. It is a common phenomenon that the ephemeral beds are generally wider, as these have to compensate the lack of depth in river-beds to carry huge volume of flood waters. Even the delta that now occurs at the mouth of the River Nara is relatively small compared to that which occurs further east in the Great Rann of Kachchh, and assigned to the River Saraswati<sup>27,28</sup>.

So much for premise 2. But we must also assess the inference from premises 1 and 2 to conclusion 3.

The First Inference (from 1 & 2 to 3): Even accepting 2(a), the inference is fallacious. If you want to prove that Harappans were really Vedic Aryans, it is going in circles to simply assume that they were Vedic Aryans... In fact, the indigenist argument usually takes recourse to an archaeological variant of the Indian Rope Trick: use first the Harappan sites to show that as Ghaggar-Hakra was presumably the cradle of Harappan civilization, then use a selective interpretation of RigVeda passages and the presumed geological facts to give the RigVeda an appropriately impressive dating ?and you will have the Harappan sites descending from the clouds as Vedic Aryan sites! All this may impress the gullible, but it is nothing but a magician?s illusion.

Premise 4: Let us accept it for the sake of the argument?

The Second Inference (from 3 & 4 to 5): We can accept this inference?

But the Mighty Saraswati has been lost already. The premise 2 is unwarranted and the usual inference to statement 3 is fallacious. Hence the argument, in the version I have presented, is not sound.

So, anyone volunteering to provide a sound argument in favour of the Vedic Aryan Saraswati being the cradle of all Indian civilization?

Best regards, Juha Savolainen

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| 98|2002-09-08 15:56:32|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > A Cautionary Brief on the Legendary Mighty Saraswati
- > River
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> But that is not all that is desired. Once one adds the  
> further premise  
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> 4. No perennial river has flowed from the Himalayas to  
> the Arabian Sea in an area that is limited from the  
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- > statement 3 is fallacious. Hence the argument, in the
- > version I have presented, is not sound.
- >

Actually you admit to premise 2(b), which in itself is something that deserves much consideration.

You even admit to premise 2(a) as a possibility but simply state that the flow of the river had to be "reduced drastically." You also infer that the Saraswati as the "greatest" river refers to massive flows relative to other rivers from the Himalayas to the sea.

But the descriptions might only be hyperbole to describe the river that sustained the life of their civilization.

Could you apply the same type of analysis to the theory that the Saraswati existed in Afghanistan?

Regards,

Paul Kekai Manansala

| 99|2002-09-09 13:04:49|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|  
Paul,

Thanks for your probing questions and comments. I do not agree with some of your comments, but I thank you for making them all the same: an excellent opportunity to clarify the way my analysis is supposed to work.

So, what is the argument? It is an idealized argument aimed at highlighting the problems many all-too-real arguments defending the existence of the 'Mighty Saraswati River' must face. 'Must' despite the fact that often these problems are hidden under streams of smug rhetoric.

To keep this exposition as easy-going as possible, I proceed by giving my comments on your thoughts in the order you made them.

PKM: 'Actually you admit to premise 2(b), which in itself is something that deserves much consideration.'

JS: No, what I actually wrote was 'There is dispute over both subpremises. However, for the sake of the argument, I will concede 2(b)'. This does not mean that I would concede 2(b), far from that! Remember, an argument is sound if and only if (i) all its premises are true and (ii) all its inferential steps are valid. Hence, to challenge the conclusion of an argument, all you have to do is to challenge successfully one of its premises or one of the inferences. It is common practice to mount such a challenge by saying 'OK, let us concede this premise/inference, just for the sake of the argument. But the argument can still be challenged because another premise is doubtful and/or another inference is invalid?' That is all.

Besides, and this is an extremely important point: Harappan sites are evidence against the 'Vedic Aryan Saraswati' thesis because Harappan sites are not 'Vedic Aryan' except in the fertile imagination of

indigenist writers. The indigenists try to avoid the impending disaster by turning the "Mighty Saraswati River" into a dating method that would push their beloved "Vedic Aryans" back in time, in fact, to times no later than the Harappan sites. After this Rope Trick, they then argue that Harappan sites must have been "Vedic Aryan", or, even "post-Vedic Aryan". Oh yes, domesticated horses, chariots, fire altars, soma and all the rest!

PKM: "You even admit to premise 2(a) as a possibility but simply state that the flow of the river had to be "reduced drastically." You also infer that the Saraswati as the "greatest" river refers to massive flows relative to other rivers from the Himalayas to the sea."

JS: No, I did not wrote that. They are citations from Jhakar and Roy:

"Even if we assume that the mighty  
> Himalayan river flowed through Rajasthan only during  
> the Middle and Late Holocene times, and was later  
> shifted westward or northwestward, the flow of the  
> river must have been reduced drastically when its  
> course merged with those now followed by the

Ghaggar,Hakra and Nara."

Anyway, what they are saying is crystal-clear: Even if river migrations had diverted the course of the "real" Saraswati River (remember, I hinted that Jhakar and Roy believe in the existence of the Mighty Saraswati somewhere in Greater Indus Valley), Ghaggar-Hakra could not have been the "Mighty Saraswati River". And yet the indigenists habitually claim that the Ghaggar-Hakra was the area where "Saraswati Civilization" flourished, because many Harappan sites are there.

Of course, being sympathetic to Mighty Saraswati seeking, Jhakar and Roy do not want to embarrass their fellow-seekers, but that is irrelevant for deducing the embarrassing consequence of their analysis of the Ghaggar-Hakra paleochannels.



Whether Jhakar's and Roy's analysis becomes the standard story is another matter. Even for the indigenists, it may become arduous to shift the sites allegedly documenting the existence of the 'Saraswati Civilization' to wherever the changing mood of the sympathetic geologists point to. However, having read quite a lot of the indigenist literature, I have no doubt that in that case a cottage industry would emerge, loudly claiming that Rigveda documents the migrations of the Mighty Saraswati etc. That always happens when religious seekers do not find their Lost Paradise in good time?

PKM: 'You also infer that the Saraswati as the "greatest" river refers to massive flows relative to other rivers from the Himalayas to the sea.

But the descriptions might only be hyperbole to describe the river that sustained the life of their civilization.'

JS: The reply is obvious. I have not made any claims about Saraswati ' except as a way of distilling the suggested argument from many and varied indigenist writings on the Mighty Saraswati. The point here is simple and important: the indigenists want to use the Mighty Saraswati as a dating device. A small rivulet called 'Saraswati' would be entirely inappropriate for such purposes, of course. Hence it is not I who is demanding that a perennial river overshadowing Indus et al. once flowed from the Himalayas to the Arabian Sea, it is the indigenists who must defend this idea. If they give up the claim, their case collapses immediately.

PKM: 'But the descriptions might only be hyperbole to describe the river that sustained the life of their civilization. '

JS: No need to comment this, because I have already explained why the indigenists must take this hyperbole literally.

PKM: 'Could you apply the same type of analysis to the

theory that the Saraswati existed in Afghanistan??

JS: I could, of course, assuming the purpose of any ?Harahvati in Afganistan?- argument would parallel to those of the indigenist argument. More precisely, if somebody were to claim that ?Vedic Aryans? must have lived in Afganistan (yes, with their horses, chariots, soma and all), say 10.000 years ago because the only period that would give a good match between the Mighty Saraswati, as described in the RigVeda, would be 10.000 years ago?well, it would be very easy to give a parallel analysis of such claims.

All in all, thanks for your questions and comments,

Best regards, Juha Savolainen

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| 100|2002-09-09 16:26:51|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > Besides, and this is an extremely important point:
- > Harappan sites are evidence against the ?Vedic Aryan
- > Saraswati? thesis because Harappan sites are not
- > ?Vedic Aryan? ? except in the fertile imagination of
- > indigenist writers.

I don't think this comment is necessary. Do the theorists who place the Saraswati in Afghanistan have any less fertile imaginations?

Even the conventional AIT/AMT proponents now admit that Vedic culture at least inherited a great deal from IVC, although they maintain the two are separate.

JS: I could, of course, assuming the purpose of any  
> ?Harahvati in Afganistan?- argument would parallel to  
> those of the indigenist argument. More precisely, if  
> somebody were to claim that ?Vedic Aryans? must have  
> lived in Afganistan (yes, with their horses, chariots,  
> soma and all), say 10.000 years ago because the only  
> period that would give a good match between the Mighty  
> Saraswati, as described in the RigVeda, would be  
> 10.000 years ago?well, it would be very easy to give a  
> parallel analysis of such claims.  
>

Even without an exact parallel could you analyze the claims that the Rgvedic Saraswati was in Afghanistan?

How does water flow, volume and "might" come into play with regard to this theory.

Regards,

Paul Kekai Manansala

| 101|2002-09-09 23:27:36|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|

My earlier post: Besides, and this is an extremely important point: Harappan sites are evidence against the ?Vedic Aryan Saraswati? thesis because Harappan sites are not ?Vedic Aryan? ? except in the fertile imagination of indigenist writers. ?

PKM: ?I don't think this comment is necessary. Do the theorists who place the Saraswati in Afghanistan have any less fertile imaginations?

Even the conventional AIT/AMT proponents now admit that Vedic culture at least inherited a great deal from IVC, although they maintain the two are

separate.?

My present comment: In the context of the indigenist Saraswati lore, my comment was highly pertinent: it is the 'otherness' of Harappan culture that has always confounded indigenist attempts to present 'Vedic Aryan' culture as the cradle of all Indian cultures. It takes a lot of rehearsing in cultural myopia to start seeing the Harappan culture as a 'Vedic Aryan' culture. Determined to see what they want, the indigenists pin their hopes on 'Mighty Saraswati' as their saviour.

As for the continuities, of course, there are continuities: some things change very slowly in human history. Clearly, the 'Vedic Aryan' culture has roots both in the Asian steppes and in India.

And Saraswati in Afganistan? See my reply below.

My earlier words: I could, of course, assuming the purpose of any 'Harahvati in Afganistan'- argument would parallel to those of the indigenist argument. More precisely, if somebody were to claim that 'Vedic Aryans' must have lived in Afganistan (yes, with their horses, chariots, soma and all), say 10.000 years ago because the only period that would give a good match between the Mighty Saraswati, as described in the RigVeda, would be 10.000 years ago?well, it would be very easy to give a parallel analysis of such claims.

PKM: 'Even without an exact parallel could you analyze the claims that the Rgvedic Saraswati was in Afghanistan?

How does water flow, volume and "might" come into play with regard to this theory.?

My present comment: Well, it all depends on the use of this legend. Are we searching for the Garden of Eden with satellite pictures and geological studies? If we are, we have not become much wiser, no matter whether

we are searching for 'Mighty Saraswati' in Afganistan or, for that matter, somewhere else in Asia. However, if we discuss matters in their proper context and recognize the difference between mytho-poetical representations and our contemporary 'universe of precision', it is quite different, no matter whether one thinks that 'Saraswati' referred to some river in Afganistan, in the Greater Indus Valley or perhaps both.

My analysis certainly does not signal any interest whatsoever in fundamentalist readings of old epic texts. No, the analysis was meant to show that the claims of the now popular indigenist school are not properly supported by the sort of evidence their establishment would call for. The nature of the 'Saraswati' river is an interesting minor detail in the larger story of Eurasian and Indian pre/history, an interesting detail blown out of all proportion by the indigenists. And my morale for the story-telling is not 'More of the same but somewhere else!' but 'Back to the real world!'

Best regards, Juha Savolainen

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| 102|2002-09-10 07:26:11|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > PKM: 'I don't think this comment is necessary. Do the
- > theorists who place the Saraswati in Afghanistan have
- > any less fertile imaginations?
- >
- > Even the conventional AIT/AMT proponents now admit
- > that Vedic culture at least inherited a great deal
- > from IVC, although they maintain the two are
- > separate.?
- >
- >

- > My present comment: In the context of the indigenist
- > Saraswati lore, my comment was highly pertinent: it is
- > the "otherness" of Harappan culture that has always
- > confounded indigenist attempts to present "Vedic
- > Aryan" culture as the cradle of all Indian cultures.
- > It takes a lot of rehearsing in cultural myopia to
- > start seeing the Harappan culture as a "Vedic Aryan"
- > culture. Determined to see what they want, the
- > indigenists pin their hopes on "Mighty Saraswati" as
- > their saviour.
- >

This doesn't show a very sound knowledge of Indian archaeology.

No culture has shown strong "Vedic Aryan" traits when one uses the Western view of chariot-bound nomads.

The Harappan culture is at least as "Vedic Aryan" as BMAC or PGW, which are most often cited by the AIT/AMT proponents.

- > How does water flow, volume and "might" come into play
- > with regard to this theory.?
- >
- >
- > My present comment: Well, it all depends on the use of
- > this legend. Are we searching for the Garden of Eden
- > with satellite pictures and geological studies? If we
- > are, we have not become much wiser, no matter whether
- > we are searching for "Mighty Saraswati" in Afganistan
- > or, for that matter, somewhere else in Asia. However,
- > if we discuss matters in their proper context and
- > recognize the difference between mytho-poetical
- > representations and our contemporary "universe of
- > precision", it is quite different, no matter whether
- > one thinks that "Saraswati" referred to some river in
- > Afganistan, in the Greater Indus Valley or perhaps
- > both.
- >

You seem to take a more literal interpretation in analyzing the "indigenest" theory. How does Afghanistan stand up using the same standard? Where is the sea (samudra) into

which the Saraswati flows? It really simply just a confluence of rivers? If so, why can't we just as flexible when judging Dr. Kalyanaraman's theory.

Regards,

Paul Kekai Manansala

| 103|2002-09-10 14:21:42|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|

- > My earlier comment: In the context of the indigenist
- > Saraswati lore, my comment was highly pertinent: it

is

- > the "otherness" of Harappan culture that has always
- > confounded indigenist attempts to present "Vedic
- > Aryan" culture as the cradle of all Indian cultures.
- > It takes a lot of rehearsing in cultural myopia to
- > start seeing the Harappan culture as a "Vedic Aryan"
- > culture. Determined to see what they want, the
- > indigenists pin their hopes on "Mighty Saraswati" as
- > their saviour.
- >

PKM: "This doesn't show a very sound knowledge of Indian archaeology"?

JS: Well, yes, my ignorance is sometimes too much to bear even for myself? Fortunately there are people like you on whose sound knowledge in all matters archaeological and geographical I have learned to take as my guide?

PKM: "No culture has shown strong "Vedic Aryan" traits when one uses the Western view of chariot-bound nomads."

JS: Sorry, I lost you here?

PKM: "The Harappan culture is at least as "Vedic Aryan" as BMAC or PGW, which are most often cited by the AIT/AMT proponents."

JS: No need to view BMAC as "Vedic Aryan", PGW may or may not have something to do with "Vedic Aryans" - at least they had domesticated horses?

PKM: ?You seem to take a more literal interpretation in analyzing the "indigenest" theory. How does Afghanistan stand up using the same standard? Where is the sea (samudra) into which the Saraswati flows? It really simply just a confluence of rivers??

JS: It is hardly my fault that indigenist literature is endlessly ruminating the ?Mighty Saraswati? as the ?scientific proof? for their amusing chronologies. I am merely taking them for task for their claims. I would imagine that anyone genuinely interested in the role of Austric-speaking people in Indian history would be prepared to follow the suite.

As for the translation of ?samudra? in this context, whether as ?ocean?, ?great body of water?, ?lake?, ?confluence of rivers?, all this is very much disputed. No need for me to demand ?Saraswati? flowing into a sea and start searching suitable rivers to match the description.

PKM: ?If so, why can't we just as flexible when judging Dr. Kalyanaraman's theory.?

JS: Paul, you seem to have forgotten already my first post on this topic. It was a plea for a sound argument in favour of ?Mighty Saraswati? being the cradle of all Indian civilization. True, familiarity with this genre has made me sceptical of such ventures. But if Kalyanaraman or anyone else do deliver, I shall certainly have second thoughts on these issues.

As for you, well, it is your messageboard and it is not only your right but also your duty to treat with respect all posters who may have something to contribute. Vanity should never prevent us from apologizing when we have ourselves made a mistake but blamed others for it. Right?

Best regards, Juha Savolainen



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| 104|2002-09-10 16:09:16|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > JS: It is hardly my fault that indigenist literature
- > is endlessly ruminating the 'Mighty Saraswati' as the
- > 'scientific proof' for their amusing chronologies. I
- > am merely taking them for task for their claims. I
- > would imagine that anyone genuinely interested in the
- > role of Austric-speaking people in Indian history
- > would be prepared to follow the suite.
- >

I am simply taking you to task for your criticism of this school of thought, which borders on ridicule. Btw, which view of Aryan origins to you subscribe to anyway?

- >
- > As for you, well, it is your messageboard and it is
- > not only your right but also your duty to treat with
- > respect all posters who may have something to
- > contribute. Vanity should never prevent us from
- > apologizing when we have ourselves made a mistake but
- > blamed others for it. Right?
- >

Do not preach to me on how to run a message board. Just stick to the subject. If I feel the need to apologize for anything, I will do so of my own accord.

You have treated the indigenous Aryan view very lightly, so why are you now crying for "respect?"

Regards,  
Paul Kekai Manansala

| 105|2002-09-11 10:47:55|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|  
PKM: ?I am simply taking you to task for your criticism of this school of thought, which borders on ridicule. Btw, which view of Aryan origins to you subscribe to anyway??

JS: I always give arguments and evidence. However, having done just that, I do occasionally poke fun at the expense of mentality that joins together (a) pontificating tone of voice, (b) chauvinistic-religious temperament and (c) disregard of contrary evidence in favour of preconceived ideas.

My view of the Aryan origins? Are you really and truly interested?

PKM: ?Do not preach to me on how to run a message board. Just stick to the subject. If I feel the need to apologize for anything, I will do so of my own accord.?

JS: Preach? - My remarks were not meant as preaching ?they were ironical reminders of your own standards of moderation. However, as this is your message board, you are fully entitled to decide what counts as ?the subject? and what not. You are even entitled to ban participants you find antithetical to your purposes. The readers, in turn, will decide whether your policy and rulings meet their expectations and whether the message board shows promise in fulfilling these expectations. In the end, you will have the message board you deserve?

PKM: ?You have treated the indigenous Aryan view very lightly, so why are you now crying for "respect"??

JS: Crying for respect? ? Sorry, I happen to enjoy vigorous debates, at least when the participants are driven by curiosity and have some sense of humour? My call for respect was not a plea of any sort for

myself, it was just a mildly ironical reminder of your own manners vis-?-vis Oscar Nalesini.

Regards, Juha Savolainen

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| 106|2002-09-11 13:01:16|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

> My view of the Aryan origins? Are you really and truly  
> interested?  
>

Yes.

>  
> PKM: ?Do not preach to me on how to run a message  
> board. Just  
> stick to the subject. If I feel the need to apologize  
> for anything, I will do so of my own accord.?  
>  
>  
> JS: Preach? - My remarks were not meant as preaching  
> ?they were ironical reminders of your own standards of  
> moderation.

This is not a moderated message board per definition. You're imagining things that don't exist. No posts are pre-checked, and so far none have been deleted. Neither is membership checked.

>  
> PKM: ?You have treated the indigenous Aryan view very  
> lightly,  
> so why are you now crying for "respect"?"

>  
>  
> JS: Crying for respect? ? Sorry, I happen to enjoy  
> vigorous debates, at least when the participants are  
> driven by curiosity and have some sense of humour?  
> My call for respect was not a plea of any sort for  
> myself, it was just a mildly ironical reminder of your  
> own manners vis-?-vis Oscar Nalesini.  
>

What does the discussion with Oscar Nalesini have to do with the present one?

Obviously, you like to poke fun, but don't like to experience any heat yourself. If you truly enjoy rigorous debate than just stick to the discussion rather than trying to act as defacto moderator yourself.

Regard,  
Paul Kekai Manansala  
| 107|2002-09-15 03:04:03|juhavs|Re: Cautionary Brief On the Mighty Saraswati|  
JS: My view of the Aryan origins? Are you really and truly interested?

PKM: "Yes."

My reply: The trail of Indo-Aryan speaking people to India is notoriously elusive. Without written documents, we must rely on the evidence of archaeology, genetics and comparative linguistics. Archaeology points clearly to Pontic steppes and southern Urals: that is where domesticated horses, chariots etc. can be found. Genetic leaves no doubt about a major flux of genes from the Pontic area to India: that is where much of the male gene pool of present Indo-European speaking caste population, especially the higher castes, comes from. And as for language, well, the prospects of an indigenist scenario do not look very promising, to put it mildly?

In brief, on a coarse-grained level, the right answer has been known for a long time. The details, however, are tantalizingly elusive and are likely to provide us major surprises.

PKM: "Do not preach to me on how to run a message board. Just stick to the subject. If I feel the need to apologize for anything, I will do so of my own accord."

JS: Preach? - My remarks were not meant as preaching  
?they were ironical reminders of your own standards of  
moderation.

PKM: "This is not a moderated message board per definition. You're  
imagining things that don't exist. No posts are pre-checked,  
and so far none have been deleted. Neither is membership  
checked."

My reply: Who owns this message board? Who has always assumed the  
role of the de facto moderator, even in the above exchanges?

PKM: "What does the discussion with Oscar Nalesini have to do  
with the present one?"

Obviously, you like to poke fun, but don't like to experience  
any heat yourself. If you truly enjoy rigorous debate  
than just stick to the discussion rather than trying  
to act as defacto moderator yourself."

My reply: If you do not view yourself as the de facto moderator of  
the board, your kind advice is pointless. You see, I greet from  
Finland and sauna-loving Finns are quite used to very high  
temperatures. No, the real problem is not any alleged heat coming  
from your direction ? it is the total lack of any light coming from  
that direction!

Regards, Juha Savolainen

| 108|2002-09-15 07:05:06|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty  
Saraswati|

- > JS: My view of the Aryan origins? Are you really and truly
- > interested?
- >
- > PKM: "Yes."
- >
- > My reply: The trail of Indo-Aryan speaking people to India is
- > notoriously elusive. Without written documents, we must rely on the
- > evidence of archaeology, genetics and comparative linguistics.
- > Archaeology points clearly to Pontic steppes and southern Urals:
- > that is where domesticated horses, chariots etc. can be found.

But chariots, etc. are never important in the Indian archaeological context.

The problem is separating fact from fanciful fiction, historical data from liturgical hymns.

It is only a few scholars that maintain that horses are not found in pre-PGW India, most connected with Michael Witzel.

In fact, anyone who has read much about Indian archaeology will find many positive identifications of horse remains by other archaeologists over the last century.

- > Genetic leaves no doubt about a major flux of genes from the Pontic
- > area to India: that is where much of the male gene pool of present
- > Indo-European speaking caste population, especially the higher
- > castes, comes from.

I've never seen any connection with the Pontic. These have been analyzed pretty closely on other lists.

All of the Y chromosome studies have been conducted in areas that don't even pertain to the "Aryan" invasion (Andhra Pradesh in S. India).

When it comes to the "Aryan invasion", genetics and Europe, there is a long dark history that has to leave us in question of motives.

How does one explain, for example, the nearly complete absence of YAP+ haplotypes in India samples as compared with Central Asian and with modern theories that this haplotype was connected with the Neolithic migration of IE speakers to Europe?

Also, with AIT/AMT theory, Vedic "history" leaves us completely in the dark. No mention of any migrations or recollection of any distant homeland at all.

There is nothing to grasp at all as compared with the Saraswati question.

Regards,  
Paul Kekai Manansala

| 109|2002-09-18 17:31:34|gymnogoy|Maps of the Ancient Sea Kings|  
This scholarly book (purchased through Amazon.com) was reprinted in the 90s and it has opened my mind. Much of the ancient world was accurately mapped long before Columbus. In fact Columbus may have obtained a copy of one of the ancient maps. These maps are believed to be the oldest human writings.

Robin Day Korea

| 110|2002-09-21 02:33:30|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|  
PKM: ?But chariots, etc. are never important in the Indian archaeological context.

The problem is separating fact from fanciful fiction, historical data from liturgical hymns.?

JS: Indeed, but chariots and domesticated horses are important for the epic ? should we not then try to find chariots and domesticated horses from places where they were commonplace ? instead of following the path of many indigenists, i.e. by imagining ?Vedic kingdoms? at the Harappan times in India?

PKM: ?It is only a few scholars that maintain that horses are not found in pre-PGW India, most connected with Michael Witzel.

In fact, anyone who has read much about Indian archaeology will find many positive identifications of horse remains by other archaeologists over the last century.?

JS: Entirely misleading. Witzel bases his views on the professional judgement of his colleague, Prof. Richard Meadow. Meadow does not just have first-hand knowledge of Harappan excavations, he happens to be an expert in zooarchaeology, i.e. having precisely the competence that is needed here. How many of the ?discoverers? of horses from Indian soil share his competence?

As for other scholars, let us pay attention to what V.N.Misra ? hardly a supporter of Witzel ? writes in his ?Prehistoric human colonization of India?

After pointing out that

?The presence of horse bones  
in the layers of the Kayatha and succeeding  
Chalcolithic  
cultures as also a terracotta figurine of a mare at  
Kayatha  
is interesting because it takes back the antiquity of  
this  
animal to the late third millennium B.C.  
mud walls and containing ash and burnt logs of wood  
has  
been identified as a sacrificial pit or yajnya kunda  
(Sankalia et al 1971).?

Misra nevertheless goes on to state that

?The PGW people cultivated rice and wheat and lived  
in wattle-and-daub houses. They were the first people  
to have definitely used the domesticated horse. The  
archaeological picture of the culture is far more  
modest  
than that depicted in the epics, the Puranas and later  
literature. The culture is dated by radiocarbon to  
1000?  
600 B.C. which again is in sharp contrast to the Hindu  
belief of the Mahabharata battle having ended before  
3000 B.C.?

PKM: ?I've never seen any connection with the Pontic.  
These have been analyzed  
pretty closely on other lists.

All of the Y chromosome studies have been conducted  
in  
areas that don't even pertain to the "Aryan" invasion  
(Andhra Pradesh in S. India).

When it comes to the "Aryan invasion", genetics and  
Europe,  
there is a long dark history that has to leave us in



question  
of motives.

How does one explain, for example, the nearly complete absence of YAP+ haplotypes in India samples as compared with Central Asian and with modern theories that this haplotype was connected with the Neolithic migration of IE speakers to Europe??

JS: All this just shows, unfortunately, that you seem to follow much more eagerly Hindutva-inspired lists than genetic research. No wonder that you are prone to misunderstand what you have read. To gain some real perspective on the Bamshad et al. results, you may want to start at

[http://hpgl.stanford.edu/publications/PNAS\\_2001\\_v98\\_p10244.pdf](http://hpgl.stanford.edu/publications/PNAS_2001_v98_p10244.pdf)

where the connection between India and Central Asia (and Eastern Europe) can easily be seen. Sorry, the indigenist comments on Bamshad et al. are worthless. As for YAP+, well, perhaps you also learn to see that raising YAP+ is irrelevant for our debate?

PKM: ?Also, with AIT/AMT theory, Vedic "history" leaves us completely in the dark. No mention of any migrations or recollection of any distant homeland at all.?

JS: Well, suppose this is entirely true. It would still prove very little: people do forget their origins.

PKM: ?There is nothing to grasp at all as compared with the Saraswati question.?

JS: I have reluctantly come to the conclusion that further discussions with you do not have much promise, not least because I now know your track record at the ?Austronesian? newsgroup. I am sorry to say but

evidently you did not learn anything from your discussions with Waruno Mahdi?

None of my critical remarks should be seen as signs of personal animosity. In fact, I am still grateful to you for some of the intriguing references and speculations. And I certainly would have very much liked to encourage you to carry on studying the pre/historic role of Austronesian and Austric-speaking people. Unfortunately, you seem to have decided that you already know what that role was. Hence your sad refusal to learn anything from your exchanges with people who might have information relevant to these issues.

Regards, Juha Savolainen

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| 111|2002-09-21 07:25:14|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

>

- > JS: Indeed, but chariots and domesticated horses are
- > important for the epic ? should we not then try to
- > find chariots and domesticated horses from places
- > where they were commonplace ? instead of following the
- > path of many indigenists, i.e. by imagining ?Vedic
- > kingdoms? at the Harappan times in India?
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Which "epic?" The Rgveda is a series of hymns that are chanted during certain rituals. It was not intended as history nor even to be taken literally at most times.

Horses and chariots are never important in the Indian archaeological context. Yet the Vedas and Epics make it seem as though armies of millions of chariots.

Not a single chariot has been associated with PGW, for example. However, images of solid wheel "chariots" have been found at Harappan sites.

So what to make of the 'mighty' armies of horses and chariots. Literacy license, maybe?

- > PKM: ?It is only a few scholars that maintain that
- > horses are
- > not found in pre-PGW India, most connected with
- > Michael
- > Witzel.
- >
- > In fact, anyone who has read much about Indian
- > archaeology
- > will find many positive identifications of horse
- > remains
- > by other archaeologists over the last century.?
- >
- > JS: Entirely misleading. Witzel bases his views on the
- > professional judgement of his colleague, Prof. Richard
- > Meadow. Meadow does not just have first-hand knowledge
- > of Harappan excavations, he happens to be an expert in
- > zooarchaeology, i.e. having precisely the competence
- > that is needed here. How many of the ?discoverers? of
- > horses from Indian soil share his competence?
- >

Meadow, a colleague of Witzel at Harvard, uses a standard beyond that of normal archaeology as he insists on the discovery of a whole horse skeleton.

Witzel himself backs up his argument by mentioning stratification issues. He basically is using a negative evidence argument, which are naturally of weak foundation.

Whole horse finds are extremely rare in Afghanistan

and in ancient post-PGW India even over periods of thousands of years. Even partial horse finds in post-PGW India are rare over many stratified layers.

So the argument is fundamentally weak.

- > ?The PGW people cultivated rice and wheat and lived
- > in wattle-and-daub houses.

Since the Rgveda does not mention wheat and it has been questioned whether it actually mentions rice, where does this leave using your standard.

- > [http://hpgl.stanford.edu/publications/PNAS\\_2001\\_v98\\_p10244.pdf](http://hpgl.stanford.edu/publications/PNAS_2001_v98_p10244.pdf)
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- > where the connection between India and Central Asia
- > (and Eastern Europe)
- > can easily be seen. Sorry, the indigenist comments on
- > Bamshad et al. are worthless.
- > As for YAP+, well, perhaps you also learn to see that
- > raising YAP+ is irrelevant for our debate?
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O.k., you seem to have taken some time to do your research.

However, if you were well versed in genetic literature, you would know that two studies have appeared since Bamshad et al. that refute the main premise of that study.

One of them is the first India-wide Y chromosome SNP study that shows that back-and-forth Y chromosome gene flow between upper and lower castes did occur.

You can find an abstract for the study in last October's issue of the American Journal of Human Genetics.

An abstract of the second study is found below. However, it appears you still need to do further research on YAP+. When you do you will find indeed that this haplotype has been associated by some very prominent researchers with IE migration into Europe.

---

Eur J Hum Genet 2001 Sep;9(9):695-700

Y-chromosome SNP haplotypes suggest evidence of gene flow among caste, tribe, and the migrant Siddi populations of Andhra Pradesh, South India.

Ramana GV, Su B, Jin L, Singh L, Wang N, Underhill P, Chakraborty R.

From observations of lack of haplotype sharing based on Y-chromosome specific short tandem repeat (STR) loci, previous reports suggested negligible gene flow among different geographic populations of India. Using Single Nucleotide Polymorphism (SNP) sites in combination with STRs, we observed evidence of haplotype sharing across caste-tribe boundaries in South India. We examined 27 SNPs in the non-recombining region of the Y chromosome to investigate gene flow in 204 individuals belonging to three caste groups (Vizag Brahmins, Peruru Brahmins, Kammas), three tribes (Bagata, Poroja, Valmiki) and an additional group (the Siddis) of African ancestry. Principal component and AMOVA analyses show that the between group component of variation is non-significant ( $P > 0.05$ ), while that among populations within the caste and tribal groups is significant ( $P$  particular, the Valmikis and Siddis are close to the caste groups. Of a total of 11 distinct SNP-haplotypes observed, the two tribal groups (Bagata and Poroja) lack the haplotypes H4, H4A, H5A and H16, which are seen in the caste groups. In contrast, all three tribal groups exhibit the Southeast Asian haplotype H11 that is absent in the caste populations. The presence of haplotypes H4, H5, H14, and H16 in the Siddis indicate that they have assimilated considerable non-African admixture. The evidence of haplotype sharing between castes and tribes is also found when the H14 lineage was further subdivided by five STR loci. We conclude that even though these SNP-based Y-haplotypes are able to distinguish the populations, gene flow in these South Indian populations is not as negligible as that inferred from other studies based on Y-specific short tandem repeat markers.

Regards,  
Paul Kekai Manansala

| 112|2002-09-22 08:20:06|a.manansala@attbi.com|Fwd: Betel chewing creeps into the New World.|  
N Z Dent J 2002 Jun;98(432):40-5

Betel chewing creeps into the New World.

Yoganathan P.

Department of Stomatology, School of Dentistry,  
University of Otago, PO Box 647, Dunedin.

Areca nut has been chewed either alone or as a component of the betel quid since ancient times. It has been estimated that more than 10 percent of the world's population chew it for its mild psychoactive effects. Betel is chewed in New Zealand and Australia by immigrants from India now dwelling in these countries. Various forms of areca nut preparations are available in Asian groceries throughout New Zealand. The regular use of betel will, in time, stain the mucosa, gums, and teeth. This habit is discouraged in many countries because of its oncogenic, addictive, and dysaesthetic properties. Dentists and other health professionals should be aware of the effects of this habit.

| 113|2002-09-23 13:12:10|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|  
JS: Indeed, but chariots and domesticated horses are  
> important for the epic ? should we not then try to  
> find chariots and domesticated horses from places  
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So what to make of the 'mighty' armies of horses and chariots. Literacy license, maybe??

My response: Do you really think that the ?millions of chariots? of RgVeda are poetic licence referring to somewhat less glorious real chariots of Harappan times in India? If you do and take the good folks of Harappa as accomplished horse-trainers you are just engaging in day-dreaming unconstrained by any real evidence. If not, you would better tell us from where and how the ?Vedic Aryans? acquired their knowledge of chariots. If no chariots were used in India, either at the Harappan times or later, it follows inevitably that the knowledge must have come from outside India, indeed, it must have come from where chariots were common. Such places, places amply documenting the invention of chariots around 2000 BCE, certainly do exist: all you have to do is to focus your attention to Southern Urals?

>

> JS: Entirely misleading. Witzel bases his views on the  
> professional judgement of his colleague, Prof.

Richard

> Meadow. Meadow does not just have first-hand

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> of Harappan excavations, he happens to be an expert

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> zooarchaeology, i.e. having precisely the competence  
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Whole horse finds are extremely rare in Afghanistan and in ancient post-PGW India even over periods of thousands of years. Even partial horse finds in post-PGW India are rare over many stratified layers.

So the argument is fundamentally weak.?

My comment: Meadow uses standards appropriate for distinguishing ?real? horses from their close kin. As for your criticism of Witzel, well, I pointed out that Misra?s published views were not much different from Witzel?s on this issue (different as their views on many other issues surely are): do you take exception to Misra as well? Anyway, you have, once again, the unenviable choice of either (a) finding domesticated horses and chariots in India, thereby trying to strengthen the local roots of RgVeda or (b) by viewing domesticated horses and chariots as literary devices referring to bygone ages, hence locating the sources of RgVeda to Urals?

?The PGW people cultivated rice and wheat and lived > in wattle-and-daub houses.

PKM: ?Since the Rgveda does not mention wheat and it has been questioned whether it actually mentions rice, where does this leave using your standard.?



My response: I certainly do not have any fast and hard views on the PGW people. They may represent a partial fusion of Indo-Aryan speaking and Munda-speaking people, but I will, as always, gladly permit further research to settle such matters.

JS:...where the connection between India and Central Asia

> (and Eastern Europe)

> can easily be seen. Sorry, the indigenist comments

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> raising YAP+ is irrelevant for our debate?

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You can find an abstract for the study in last October's

issue of the American Journal of Human Genetics.?

My response: What article is that? You see, I do have both a digital and a Gutenbergian access to the full AJHG articles, but not from my home computer. Could you please provide the precise reference so that I can check whether I have read the article or not.

PKM: ?An abstract of the second study is found below.

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researchers with IE migration into Europe.?

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following the footsteps of Renfrew et al. who believe  
that the Indo-European languages spread from the Near  
East via demic diffusion. Well, the idea of demic  
agricultural diffusion in itself has now received more  
support, but I am not aware of having supported the  
idea that this spread signals the diffusion of  
Indo-European language(s) from their homeland in Near  
East. Hence your reference to YAP+, in its context,  
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PKM:

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Y-chromosome SNP haplotypes suggest evidence of gene  
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Ramana GV, Su B, Jin L, Singh L, Wang N, Underhill P,  
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Y-chromosome specific short tandem repeat (STR) loci,  
previous reports suggested negligible gene flow among  
different geographic populations of India. Using  
Single  
Nucleotide Polymorphism (SNP) sites in combination  
with  
STRs, we observed evidence of haplotype sharing across  
  
caste-tribe boundaries in South India. We examined 27  
SNPs in the non-recombining region of the Y chromosome

to investigate gene flow in 204 individuals belonging to three caste groups (Vizag Brahmins, Peruru Brahmins, Kammas), three tribes (Bagata, Poroja, Valmiki) and an

additional group (the Siddis) of African ancestry. Principal component and AMOVA analyses show that the between group component of variation is non-significant ( $P > 0.05$ ), while that among populations within the caste and tribal groups is significant (P particular, the Valmikis and Siddis are close to the caste groups. Of a total of 11 distinct SNP-haplotypes

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that they have assimilated considerable non-African admixture. The evidence of haplotype sharing between castes and tribes is also found when the H14 lineage was further subdivided by five STR loci. We conclude that even though these SNP-based Y-haplotypes are able

to distinguish the populations, gene flow in these South Indian populations is not as negligible as that inferred from other studies based on Y-specific short tandem repeat markers.?

My response: Sorry, I know all this research? Your interpretation of it is most amusing: none of the authors gives any indications of trying to refute Bamshad et al (2001)? What they are saying is that the findings of Bamshad et al. (1997) and Bhattacharyaa et al (1998) of only female gene flow between castes do not cover all cases and that inference to "no male gene flow between castes" would not be justified. No attempt is made to challenge the results of Bamshad et al (2001). In fact, if there were no gene flow, the observed results would be even more dramatic.

Indeed, the only way the Ramana et al (2001) study could jeopardize Bamshad et al. (2001) would be by showing that the gene flow between castes (and between caste population and tribals) would have been so strong as to turn caste membership irrelevant as a marker. One does not need to be a geneticist to see that this is certainly not the case. Indeed, if you actually bother to read the study you cited, you will notice that (unlike the caste populations) of the tribals only the Valmiki shows any evidence of haplogroup 16?

Remember what I said of the proper perspective on Bamshad et al results? While it is true that studies of populations living in different areas of India should follow, the prognosis for the indigenist hopes looks very gloomy...

In brief, your attempted ?refutation? of Bamshad et al (2001) is just clutching at straws. But then, no other indigenist has come up with anything better?

Regards,  
Juha Savolainen

---

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| 114|2002-09-23 15:41:33|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > If no chariots were used in India, either at the
- > Harappan times or later, it follows inevitably that
- > the knowledge must have come from outside India,
- > indeed, it must have come from where chariots were
- > common.

I never said India had no knowledge of chariots in Harappan times. In fact, there is some evidence of solid wheel chariots.

There is never any evidence of millions of chariots, though.  
And no evidence of chariots in PGW culture or BMAC culture.

Simply put, the same logic of your pedantic "mighty Saraswati" argument is also applicable to a "mighty chariot army" argument against any literal interpretation of the Vedas.

- > PKM: ?Meadow, a colleague of Witzel at Harvard, uses a
- > standard
- > beyond that of normal archaeology as he insists on the
- >
- > discovery of a whole horse skeleton.
- >
- > Witzel himself backs up his argument by mentioning
- > stratification issues. He basically is using a
- > negative
- > evidence argument, which are naturally of weak
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- > Whole horse finds are extremely rare in Afghanistan
- > and in ancient post-PGW India even over periods of
- > thousands of years. Even partial horse finds in
- > post-PGW
- > India are rare over many stratified layers.
- >
- > So the argument is fundamentally weak.?
- >
- > My comment: Meadow uses standards appropriate for
- > distinguishing ?real? horses from their close kin.

Meadow on more than one occasion admits simply that no positive identification can be made one way or another. And he uses the whole horse skeleton as his justification. That's beyond the normal standard for this type of research.

Besides there are strong biological arguments against the idea that the Indian horse is a relative recent (Copper Age?) introduction from Central Asia.

>  
 > ?The PGW people cultivated rice and wheat and lived  
 > > in wattle-and-daub houses.  
 >  
 > PKM: ?Since the Rgveda does not mention wheat and it  
 > has been questioned  
 > whether it actually mentions rice, where does this  
 > leave  
 > using your standard.?  
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 > My response: I certainly do not have any fast and hard  
 > views on the PGW people. They may represent a partial  
 > fusion of Indo-Aryan speaking and Munda-speaking  
 > people, but I will, as always, gladly permit further  
 > research to settle such matters.  
 >

You may have missed one important point. How could invading Aryans not know about wheat? Doesn't that go against all standard theories of PIE culture?

> >  
 >  
 > PKM: ?O.k., you seem to have taken some time to do  
 > your research.  
 >  
 > However, if you were well versed in genetic  
 > literature,  
 > you would know that two studies have appeared since  
 > Bamshad et al. that refute the main premise of that  
 > study.  
 >  
 > One of them is the first India-wide Y chromosome SNP  
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 > You can find an abstract for the study in last  
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 > check whether I have read the article or not.  
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 > PKM: ?An abstract of the second study is found below.  
 > However,  
 > it appears you still need to do further research on  
 > YAP+. When you do you will find indeed that this  
 > haplotype has been associated by some very prominent  
 > researchers with IE migration into Europe.?  
 >  
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 > My comment: As for YAP+, well, you are clearly  
 > following the footsteps of Renfrew et al. who believe  
 > that the Indo-European languages spread from the Near  
 > East via demic diffusion. Well, the idea of demic  
 > agricultural diffusion in itself has now received more  
 > support, but I am not aware of having supported the  
 > idea that this spread signals the diffusion of  
 > Indo-European language(s) from their homeland in Near  
 > East. Hence your reference to YAP+, in its context,  
 > was and remains as irrelevant.  
 >

No, it's obvious here that you don't know the research,  
 just as you're unaware of the conference paper I mentioned  
 above. I'm referring to genetic research not  
 Renfrew.

Again, take your time and research the index of the  
 conference papers from the Oct. 2001 issue of AJHG.

It's ok if it takes you a couple of days.

>  
 > PKM:  
 >  
 > ?Eur J Hum Genet 2001 Sep;9(9):695-700  
 >  
 > Y-chromosome SNP haplotypes suggest evidence of gene  
 > flow among caste,  
 > tribe, and the migrant Siddi populations of Andhra  
 > Pradesh, South  
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> Chakraborty R.

>

>

>

>

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> interpretation of it is most amusing: none of the  
> authors gives any indications of trying to refute  
> Bamshad et al (2001)? What they are saying is that the  
> findings of Bamshad et al. (1997) and Bhattacharyaa et  
> al (1998) of only female gene flow between castes do  
> not cover all cases and that inference to "no male  
> gene flow between castes" would not be justified. No  
> attempt is made to challenge the results of Bamshad et  
> al (2001). In fact, if there were no gene flow, the  
> observed results would be even more dramatic.

>

> Indeed, the only way the Ramana et al (2001) study  
> could jeopardize Bamshad et al. (2001) would be by  
> showing that the gene flow between castes (and between  
> caste population and tribals) would have been so  
> strong as to turn caste membership irrelevant as a  
> marker. One does not need to be a geneticist to see  
> that this is certainly not the case. Indeed, if you  
> actually bother to read the study you cited, you will  
> notice that (unlike the caste populations) of the  
> tribals only the Valmiki shows any evidence of  
> haplogroup 16?

Well I have read the article as any investigation of the  
Indian Civilization archives and other group archives will  
show. I've been discussing this for some time now with  
people who actually do know the research.

Any significant gene flow between male castes demolishes  
Bamshad et al. Since one cannot infer where any "markers"  
originate. This has been clearly shown in Kivisild et al.  
with regard to mtDNA in India.

Besides the Bamshad et al. itself is flawed without other  
research showing substantial male gene flow between  
caste boundaries.



First of all, it studies modern DNA in South India and tries to apply this to a proposed historical period thousands of years ago in NW India. All of the genetic samples come from some unidentified "Kshatriya" caste from Andhra Pradesh.

Since there were many migration of NW Indians, including the Moguls and their allies, to South India during Muslim times, it would be interesting to note the identity of this "Kshatriya" caste.

Of course, NW India received substantial migration from Central Asian during Turkic and Mogul times, so any caste coming from there to South India might naturally seem closer to Central Asians.

There is even a theory that quite a number of brahmin castes are of Persian or Arabian origin including the Konkanastha brahmins.

Either way, the brahmin caste appeared more similar to Asians than Europeans in Bamshad et al. which is contradictory to his arguments.

Bamshad et al. relies almost entirely on the evidence given by some unidentified "Kshatriya" caste from South India in comparison with other South Indians.

The study shows absolutely no knowledge of Indian caste history when it suggests that the Kshatriya caste was somehow defined biologically.

Anyone familiar with Indian history would know that there have been many foreign Kshatriya castes created over the ages including the Saka, Cina, Yavana, Kirata etc.

Regards,  
Paul Kekai Manansala  
| 115|2002-09-23 15:58:10|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

>

All of the genetic

> samples come from some unidentified "Kshatriya" caste  
> from Andhra Pradesh.  
>

This should be all of the relevant upper caste samples since the brahmin samples did not support the authors' thesis.

Regards,

Paul Kekai Manansala

| 116|2002-09-28 08:37:50|a.manansala@attbi.com|Fwd: New findings help archaeologists reveal nations history|

[http://www.vov.org.vn/2002\\_09\\_27/english/vanhoal.htm](http://www.vov.org.vn/2002_09_27/english/vanhoal.htm)

VOV News

New findings help archaeologists reveal nation's history

Reports show Vietnam's archaeologists have made outstanding progress in archaeological findings this year. At a two-day conference in Hanoi to review the sector's achievements, archaeologists said metal relics of Vietnamese archaeological value with dozens of bronze drums have been found in the country from northern Lai Chau province to southern Ben Tre province, including eight in Thanh Hoa province.

They found Palaeozoic fossils of ancient people and Rhinoceros' teeth dating from the Stone Age at Ma Uoi excavation site in northern Hoa Binh province. Hundreds of remains of earthenware and dozens of stone axes and spearheaded tools were unearthed in Lang Cong (north-central Thanh Hoa province) and Va Vung (north-eastern Quang Ninh province).

In addition, many ancient tombs and traces of kilns, bronze relics and earthenware of the Dong Dau, Dong Son culture, and iron and glass dating back to the first century AD were found in Go Cam, central Quang Nam province. Hundreds of relics dating back to 3,200 years ago were unearthed in Rach La, southern Dong Nai province. Finally, hundreds of architectural stone blocks, stilts from stilt-houses and other relics of the Champa-Oc Eo culture were excavated in Nhan Thanh, southern Can Tho province, and Khe The, Quang Nam

province.

These discoveries are of great significance for the research of the country's history and culture. Deputy Director of the National Archaeology Institute, Doctor Ha Van Hung said, "Vietnamese archaeologists this year have excavated more than 20 sites and unearthed thousands of relics. The new discoveries help enrich exhibits displayed at local museums, thus contributing to preserving and promoting the country's cultural identity."

The conference concluded on Friday.

| 117|2002-09-28 14:10:00|Juha Savolainen|Re: Cautionary Brief On the Mighty Saraswati|  
Reply to Paul Kekai Manansala  
(because of the great length of this post, I will send the Part II later)

#### Part I: The Genetic Evidence

This is an edited version of the debate between Paul Kekai Manansala and myself on the genetic evidence concerning the controversial "Aryan origins" question. I ask the readers of this newsgroup to pay close attention to what the controversial question actually was and what sort of evidence has been provided to support the claims.

JS: My view of the Aryan origins? Are you really and truly interested?

PKM: "Yes."

JS: The trail of Indo-Aryan speaking people to India is notoriously elusive. Without written documents, we must rely on the evidence of archaeology, genetics and comparative linguistics.

"Genetics leaves no doubt about a major flux of genes from the Pontic area to India: that is where much of the male gene pool of present Indo-European speaking caste population, especially the higher castes, comes from?"

PKM: "I've never seen any connection with the Pontic."

These have been analyzed pretty closely on other lists.

All of the Y chromosome studies have been conducted in areas that don't even pertain to the "Aryan" invasion (Andhra Pradesh in S. India).

When it comes to the "Aryan invasion", genetics and Europe, there is a long dark history that has to leave us in question of motives.

How does one explain, for example, the nearly complete absence of YAP+ haplotypes in India samples as compared with Central Asian and with modern theories that this haplotype was connected with the Neolithic migration of IE speakers to Europe???

JS: All this just shows, unfortunately, that you seem to follow much more eagerly Hindutva-inspired lists than genetic research. No wonder that you are prone to misunderstand what you have read. To gain some real perspective on the Bamshad et al. results, you may want to start at

[http://hpgl.stanford.edu/publications/PNAS\\_2001\\_v98\\_p10244.pdf](http://hpgl.stanford.edu/publications/PNAS_2001_v98_p10244.pdf)

where the connection between India and Central Asia (and Eastern Europe) can easily be seen. Sorry, the indigenist comments on Bamshad et al. are worthless. As for YAP+, well, perhaps you also learn to see that raising YAP+ is irrelevant for our debate?

PKM:??O.k., you seem to have taken some time to do your research.

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From observations of lack of haplotype sharing based on Y-chromosome specific short tandem repeat (STR) loci, previous reports suggested negligible gene flow among different geographic populations of India. Using Single Nucleotide Polymorphism (SNP) sites in combination with STRs, we observed evidence of haplotype sharing across caste-tribe boundaries in South India. We examined 27 SNPs in the non-recombining region of the Y chromosome to investigate gene flow in 204 individuals belonging to three caste groups (Vizag Brahmins, Peruru Brahmins, Kammas), three tribes (Bagata, Poroja, Valmiki) and an

additional group (the Siddis) of African ancestry. Principal component and AMOVA analyses show that the between group component of variation is non-significant ( $P > 0.05$ ), while that among populations within the caste and tribal groups is significant ( $P$

close to the caste groups. Of a total of 11 distinct SNP-haplotypes observed, the two tribal groups (Bagata and Poroja) lack the haplotypes H4, H4A, H5A and H16, which are seen in the caste groups. In contrast, all three tribal groups exhibit the Southeast Asian haplotype H11 that is absent in the caste populations. The presence of haplotypes H4, H5, H14, and H16 in the Siddis indicate that they have assimilated considerable non-African admixture. The evidence of haplotype sharing between castes and tribes is also found when the H14 lineage was further subdivided by

five STR loci. We conclude that even though these SNP-based Y-haplotypes are able to distinguish the populations, gene flow in these South Indian populations is not as negligible as that inferred from other studies based on Y-specific short tandem repeat markers.?

JS: What article is that? You see, I do have both a digital and a Gutenbergian access to the full AJHG articles, but not from my home computer. Could you please provide the precise reference so that I can check whether I have read the article or not.

As for YAP+, well, you are clearly following the footsteps of Renfrew et al. who believe that the Indo-European languages spread from the Near East via demic diffusion. Well, the idea of demic agricultural diffusion in itself has now received more support, but I am not aware of having supported the idea that this spread signals the diffusion of Indo-European language(s) from their homeland in Near East. Hence your reference to YAP+, in its context, was and remains as irrelevant.

Sorry, I know all this research?  
Your interpretation of it is most amusing: none of the authors gives any indications of trying to refute Bamshad et al (2001)? What they are saying is that the findings of Bamshad et al. (1997) and Bhattacharyaa et al (1998) of only female gene flow between castes do not cover all cases and that inference to "no male gene flow between castes" would not be justified. No attempt is made to challenge the results of Bamshad et al (2001). In fact, if there were no gene flow, the observed results would be even more dramatic.

Indeed, the only way the Ramana et al (2001) study could jeopardize Bamshad et al. (2001) would be by showing that the gene flow between castes (and between caste population and tribals) would have been so strong as to turn caste membership irrelevant as a marker. One does not need to be a geneticist to see that this is certainly not the case. Indeed, if you actually bother to read the study you cited, you will notice that (unlike the caste populations) of the tribals only the Valmiki shows any evidence of

haplogroup 16?

Remember what I said of the proper perspective on Bamshad et al results? While it is true that studies of populations living in different areas of India should follow, the prognosis for the indigenist hopes looks very gloomy...

In brief, your attempted ?refutation? of Bamshad et al (2001) is just clutching at straws. But then, no other indigenist has come up with anything better?

PKM: ?No, it's obvious here that you don't know the research, just as you're unaware of the conference paper I mentioned above. I'm referring to genetic research not Renfrew.

Again, take your time and research the index of the conference papers from the Oct. 2001 issue of AJHG.

It's ok if it takes you a couple of days.

Well I have read the article as any investigation of the Indian Civilization archives and other group archives will show. I've been discussing this for some time now with people who actually do know the research.

Any significant gene flow between male castes demolishes Bamshad et al. Since one cannot infer where any "markers" originate. This has been clearly shown in Kivisild et al. with regard to mtdna in India.

Besides the Bamshad et al. itself is flawed without other research showing substantial male gene flow between caste boundaries.

First of all, it studies modern DNA in South India and tries to apply this to a proposed historical period thousands of years ago in NW India. All of the genetic samples come from some unidentified "Kshatriya" caste from Andhra Pradesh.

Since there were many migration of NW Indians, including the Moguls and their allies, to South India during Muslim times, it would be interesting to note the identity of this "Kshatriya" caste.

Of course, NW India received substantial migration from Central Asian during Turkic and Mogul times, so any caste coming from there to South India might naturally seem closer to Central Asians.

There is even a theory that quite a number of brahmin castes are of Persian or Arabian origin including the Konkanastha brahmins.

Either way, the brahmin caste appeared more similar to Asians than Europeans in Bamshad et al. which is contradictory to his arguments.

Bamshad et al. relies almost entirely on the evidence given by some unidentified "Kshatriya" caste from South India in comparison with other South Indians.

The study shows absolutely no knowledge of Indian caste history when it suggests that the Kshatriya caste was somehow defined biologically.

Anyone familiar with Indian history would know that there have been many foreign Kshatriya castes created over the ages including the Saka, Cina, Yavana, Kirata etc.?

JS: (1) Does the Ramana1, Su1 et al. (2001) refute the Bamshad, Kivisild et al. (2001)? Well, while stating that

?Earlier studies from India, based on Y chromosome short tandem repeat (STR) polymorphisms have shown that there is either negligible or no male gene flow among populations of India.<sup>7,8</sup> In contrast, mtDNA d-loop sequence variation<sup>7</sup> showed higher levels of female gene flow between related caste groups. In this research article, we provide new data on 27 Y-chromosome SNP sites in three castes, three tribes, and Siddis (a migrant population of African ancestry) of Andhra Pradesh, South India, and demonstrate that while these SNP markers reveal a



substantial genetic variation among these groups, they also detect an evidence of male gene flow among these population groups.?

Ramana, Su et al. study (2001) also points out that

?The principal component analysis (Figure 2) of the haplotype distributions reveals that more than 87% of the haplotype variation is explained by the three principal components. The positions of the populations, by the three principal component scores, do not generally cluster them by their caste or tribal affiliation. For example, the Valmiki are closest to the Peruru Brahmins, both of which are also close to the Siddis (particularly based on the first two principal components). Also, the Vizag Brahmins and Peruru Brahmins are distant from each other (particularly based on PC1), although they belong to the same social hierarchy.?

Now, clearly, these results do challenge an assumption Bamshad, Kivisild et al. use in interpreting the famous results published in their ?Genetic Evidence on the Origins of Indian Caste Populations? in ?Genome Research? (Vol. 11, Issue 6, 994-1004, June 2001):

?The high affinity of caste Y chromosomes with those of Europeans suggests that the majority of immigrating West Eurasians may have been males. As might be expected if West Eurasian males appropriated the highest positions in the caste system, the upper caste group exhibits a lower genetic distance to Europeans than the middle or lower castes. This is underscored by the observation that the Kshatriya (an upper caste), whose members served as warriors, are closer to Europeans than any other caste (data not shown). Furthermore, the 32-bp deletion polymorphism in CC chemokine receptor 5, whose frequency peaks in populations of Eastern Europe, is found only in two Brahmin males (M. Bamshad and S.K. Ahuja, unpubl.). The stratification of Y-chromosome distances with Europeans could also be caused by male-specific gene flow among caste populations of different rank. However, we and others have demonstrated that there is little sharing of Y-chromosome haplotypes among castes of different rank (Bamshad et al. 1998 ; Bhattacharyya

et al. 1999 ).?

But notice in what sense the Ramana, Su et al. (2001) study actually can be viewed as challenging the Bamshad, Kivisild et al. (2001): the former can be seen as challenging the Bamshad, Kivisild et al. account of the history of the stratification of the Y-chromosome distances of caste populations with Europeans. Now, pointing out that is quite fair in itself - and certainly relevant for the purposes of Bamshad, Kivisild et al. (2001). As for me, I regret that I did not make my point with sufficient clarity when I wrote that:

?Your interpretation of it is most amusing: none of the authors gives any indications of trying to refute Bamshad et al (2001)? What they are saying is that the findings of Bamshad et al. (1997) and Bhattacharyaa et al (1998) of only female gene flow between castes do not cover all cases and that inference to "no male gene flow between castes" would not be justified. No attempt is made to challenge the results of Bamshad et al (2001). In fact, if there were no gene flow, the observed results would be even more dramatic.

Indeed, the only way the Ramana et al (2001) study could jeopardize Bamshad et al. (2001) would be by showing that the gene flow between castes (and between caste population and tribals) would have been so strong as to turn caste membership irrelevant as a marker.?

To be sure, I think my meaning can be seen and understood in the context, but in debates such as this it is imperative to be as clear and translucent as possible. So, I rephrase my point as follows:

?Look, what we are debating on is whether there was a major flux of genes from the Pontic area to India and whether much of the male gene pool of present Indo-European speaking caste population, especially the higher castes, comes from that area. If I am correct and the origins of ?Vedic Aryan? culture and language point to the Pontic area/Central Asia, it follows that there should also be observable traces of the history of ?Vedic Aryan? people, preceding their

entry to India, in the genes of those Indians who are their descendants. If many of their fathers once lived in the Pontic steppes, they should still carry traces of genes originating from there and we should find these traces mainly in the upper castes because the arriving Indo-European speaking people certainly contributed to the establishment of the hierarchic caste system. Sure, the complicated history of India means many complications in the available evidence, but the general picture should give support to the thesis. On the other hand, if the indigenists are correct, (a) we should not find more than a marginal amount of Y-chromosomes of the Pontic area (originating during the Late Glacial/early Holocene period and spreading to India much later but earlier than 1000 BCE) in the Indian gene pool and (b) we should not find correlations between the carriers of these markers and their Indo-European language and higher position in the caste. Rather, we should expect that (a) these genes would be generally absent among Hindi-speaking upper caste people and/or (b) their arrival times would suggest later invasions or migrations during the historical period and/or (c) they could be found as easily among the Austric-speaking tribals than among the Brahmins of Uttar Pradesh??

This is what I have been defending from the very beginning of our debate. Hence the issue of gene flow versus no gene flow is only of minor relevancy for my purposes. To see the point, one only has to remember what Partha Majumder wrote when he commented the Bamshad, Kivisild et al. study (2001) in the very same number of *Genome Research*. Majumder wrote:

"The use of "upper", "middle", and "lower" to designate caste hierarchy is much more recent than the use of varna. Whereas varnas are traditionally defined, different anthropologists have used different definitions of upper, middle, and lower castes, in terms of the castes that they included in each of these clusters. Sometimes these differences in definitions have stemmed from socio-cultural similarities or differences as noted or perceived by different anthropologists, and sometimes ranked caste-cluster compositions were altered for

convenience, such as pooling to adjust for small sample sizes. As noted earlier, in studies such as Bamshad et al.'s, the most appropriate classification is by varna. As the reader will note, the authors have analyzed their data using different compositions of hierarchical caste-clusters and have obtained homologous results. However, it needs to be emphasized that traditional varna system is the only unequivocally accepted hierarchical system. In studies pertaining to the origins of castes, one is liable to draw incorrect inferences by including castes belonging to different varnas in the same ranked cluster.?

Pointing out this certainly does not mean that Majumder is here defending the indigenist view, it merely means that Majumder (quite sensibly, in fact), while commenting on the Bamshad, Kivisild et al. (2001) choice of populations living geographically close together, reminds us of important facts related to the social history of castes:

?Bamshad et al. have chosen to study caste populations drawn from a restricted geographical region of India. They have rightly emphasized the need to replicate their findings. This is absolutely essential because, as Karve (1961) has noted, "it is not generally realized that the caste society in a sense was a very elastic society." Indeed, a caste bearing the same name may have very different origins in different geographical regions. There are examples in which a tribe dispersed over a large geographical region, took up different occupations in different sub-regions, and "fitted" itself into the caste hierarchy on different rungs. Karve's work has also indicated that each of the different Brahmin castes (Chitpavan, Sarasvat, etc.) in Maharashtra probably has a different origin. Thus, the origin of caste populations may not be uniform over the entire India geographical space, and it is crucial to undertake studies to replicate Bamshad et al.'s findings. Finally, I would also like to suggest that in future studies bearing on the origins of the Indian castes, it would be a good idea to include tribal populations inhabiting the same region along with the caste populations. ?

I agree with these sentiments because they correspond with my own understanding of Indian history. However, in view of the indigenists' repeated attempts to sow confusion on this issue, it is worth pointing out that (a) it is not enough just to appeal to gene flow as an alternative explanation of the results of the Bamshad, Kivisild et al. (2001) study, one must also show the specific nature of that gene flow and to document it by appropriate evidence and (b) the debate on the gene flow versus no gene flow concerns only the history of the 'Indo-European' Y-chromosomes after their arrival in India, not of their origins. To repeat, what I have claimed is this:

'Genetics leaves no doubt about a major flux of genes from the Pontic area to India: that is where much of the male gene pool of present Indo-European speaking caste population, especially the higher castes, comes from'?

In the light of present evidence, my view is simply quite correct. The importance of Bamshad, Kivisild et al. (2001) for me is not in their account of the history of the caste hierarchy, but in the compelling evidence they provide for my view. Indeed, I could quote also Ramana, Su et al. (2001) for showing that

'Haplotype sharing and frequency differences of haplotypes can be examined in the light of these observations. It is true that the caste populations (both Brahmin groups and the Kammas) can be distinguished from the two tribal groups (Bagata, Poroja), since the caste populations exhibit the haplotypes H4, H4A, H5A and H16, which are not present in the two tribal groups. In contrast, all the tribal groups show the presence of the Southeast Asian specific haplotype H11.12,13 However, the Valmikiis share haplotypes H1A, H4,H4A, H5A and H16 with caste populations. They also exhibit the Southeast Asian haplotype H11, which is present in the other two tribes, but neither in any of the caste populations nor in Siddis.'

and that

'Our study on the haplotypic diversity based on Y

chromosome SNPs demonstrates that the caste and tribal populations of Andhra Pradesh, South India can be distinguished by the presence of some haplotypes that are unique to these groups (H4, H4A, H5A, and H16 in the caste groups, and H11 in the tribals). However, the presence of haplotypes H4B, H5, and H14 in all of caste and tribal groups studied, and the presence of haplotypes H4A, H5A, H14 and H16 in the Valmiki raise the possibility of extensive gene flow across the caste-tribe distinction of populations in this region of the country.?

More to the point, the haplogroup 16 (defined by M17) is represented only in the Valmiki tribals but it is represented in all caste populations that were studied (although in the clear minority of cases) ? M17 being an ?Indo-European? marker originating from the Pontic steppes.

(2) You claim that ?one cannot infer where any "markers" originate. This has been clearly shown in Kivisild et al. with regard to mtDNA in India.?

- Could you please give your reasons for making this astonishing claim?

You also state that

?First of all, it studies modern DNA in South India and tries to apply this to a proposed historical period thousands of years ago in NW India. All of the genetic samples come from some unidentified "Kshatriya" caste from Andhra Pradesh.?

- Do you think that the modern DNA from NW India would not show clear evidence of Y-chromosomes that are ?Indo-European? markers? Indeed, did you read my reference

[http://hpgl.stanford.edu/publications/PNAS\\_2001\\_v98\\_p10244.pdf](http://hpgl.stanford.edu/publications/PNAS_2001_v98_p10244.pdf)

?The Eurasian Heartland: A continental perspective on Y-chromosome diversity?

R. Spencer Wells,a,b, Nadira Yuldasheva,c, Ruslan

Ruzibakiev, Peter A. Underhill, Irina Evseeva,  
Jason Blue-Smith, Li Jin, Bing Su, Ramasamy  
Pitchappan, Sadagopal Shanmugalakshmi, Karupiah  
Balakrishnan, Mark Read, Nathaniel M. Pearson,  
Tatiana Zerjal, Matthew T. Webster, Irakli  
Zholoshvili, Elena Jamarjashvili,  
Spartak Gamburov, Behrouz Nikbinn, Ashur Dostiev,  
Ogonazar Aknazarov, Pierre Zalloua, Igor Tsoyr,  
Mikhail Kitaev, Mirsaid Mirrahimov, Ashir Chariev,  
and Walter F. Bodmer, et al.

The current distribution of the M17 haplotype is likely to represent traces of an ancient population migration originating in southern Russia/Ukraine, where M17 is found at high frequency (50%). It is possible that the domestication of the horse in this region around 3,000 B.C. may have driven the migration (27). The distribution and age of M17 in Europe (17) and Central/Southern Asia is consistent with the inferred movements of these people, who left a clear pattern of archaeological remains known as the Kurgan culture, and are thought to have spoken an early Indo-European language (27, 28, 29). The decrease in frequency eastward across Siberia to the Altai-Sayan mountains (represented by the Tuvian population) and Mongolia, and southward into India, overlaps exactly with the inferred migrations of the Indo-Iranians during the period 3,000 to 1,000 B.C. (27). It is worth noting that the Indo-European speaking Saurashtrians, a population from Tamil Nadu in southern India, have a much higher frequency of M17 than their Dravidian-speaking neighbors, the Yadavas and Kallars (39% vs. 13% and 4%, respectively), adding to the evidence that M17 is a diagnostic Indo-Iranian marker. The exceptionally high frequencies of this marker in the Kyrgyz, Tajik Khojant, and Ishkashim populations are likely to be due to drift, as these populations are less diverse, and are characterized by relatively small numbers of individuals living in isolated mountain valleys.

as somehow suggesting that the Indo-European markers would be found only in South India?

And you go on, unfortunately, to explain that

?Since there were many migration of NW Indians, including the Moguls and their allies, to South India during Muslim times, it would be interesting to note the identity of this "Kshatriya" caste.

Of course, NW India received substantial migration from Central Asian during Turkic and Mogul times, so any caste coming from there to South India might naturally seem closer to Central Asians.?

- Well, evidently the estimates, made by geneticists by using standard methods, on the arrival times of the relevant Y-chromosomes to India mean nothing to you?Or might it be the case that you and your anonymous specialists have never seen such estimates?

(3) And now the confession section. It is always dangerous to claim that ?I know all this?: you were quite correct in pointing out that I had overlooked the poster abstract by Indugula, Mastana et al. (AJHG, October 2001, conference section). I thank you for your reference to this abstract, not merely because it showed the folly of my hubris but mainly because the research mentioned in the poster abstract is very intriguing and seems to support the conclusions of Ramana, Su et al. However, the final sentence of poster actually says that

?Our initial observations are suggestive of past population movements within the subcontinent before the formation of the Hindu caste system together with population migrations from various routes contributing to the current complex population structure.?

Compare with Ramana, Su et al.(2001)

?A longer antiquity of haplotypes, as compared to formation of caste and tribal groups, may be proposed to explain the observation of SNP-haplotype sharing of the Valmiki with the caste populations. Two lines of evidence suggest that this may not be the case. First, the non-significant group differences of SNP-haplotype diversity as well as STR haplotype sharing between the castes and tribes (Tables 2 and 3) suggest evidence of gene flow across caste-tribe boundaries, rather than antiquity of



haplotypes. Second, Underhill et al,<sup>4</sup> estimated that the average time of adding a new mutation in the non-recombining region of the Y chromosome is approximately 6900 years, which places H14 to have evolved (with three mutations) 20 700 years after H1, and H16 (with five mutations) 34 500 years after H1. With H1 estimated to be 44 000 years old,<sup>4</sup> these may indicate that H14 and H16 may have existed at a time predating the separation of caste and tribes in India.<sup>14</sup> However, we observed haplotype sharing between castes and tribes at the STR level as well within the H14 lineage (Table 3), some of which are at least two mutation steps different from each other. The non-significant caste-tribe group difference of the STR-haplotypes of the H14 lineage supports the gene flow hypothesis rather than the antiquity of the haplotypes.?

Note that the conclusion by Indugula, Mastana et al. poster refers to an alternative to gene flow, namely, to haplotype sharing before the emergence of the Hindu caste system. By saying this, I do not mean that Indugula, Mastana et al. would contradict Ramana, Su et al. study (2001). No, my point is simply this: Indugula, Mastana et al. is an abstract of a poster, not an abstract of a published article. Hence it is problematic to refer to it because there is no way to make an independent study of its contents. Of course, if you do possess a full article by Indugula, Mastana et al. that gives the lacking details, you are much praised when telling where it can be found.

Concluding comments: I do not accept the ideas of geneticists uncritically. They do sometimes make themselves guilty to defective reasoning that resembles Saraswati story-telling. However, the actual results the geneticists have unearthed are quite sufficient to show the reality of a gene flow from Pontic steppes/Central Asia to India more during the Holocene era but before 1000 BCE. Real debate on this score is already over. The real debate concerns on the time frame of this (these) fluxes and their circumstances.

Regards, Juha Savolainen

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| 118|2002-09-28 15:09:46|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty  
Saraswati|

I think for the sake of brevity, you should only include  
quoted text that is relevant to the present discussion.

Otherwise there is more quoted text than response text,  
which can be burdensome to people with email box  
quotas.

Readers can look at the archives for older parts of the  
discussion.

- > Anyone familiar with Indian history would know that
- > there have been many foreign Kshatriya castes created
- > over the ages including the Saka, Cina, Yavana, Kirata
- > etc.?
- >
- > JS: (1) Does the Ramana1, Su1 et al. (2001) refute the
- > Bamshad, Kivisild et al. (2001)? Well, while stating
- > that
- >

What Kivisild et al. showed was that markers alone are useless unless you can show some chronology that matches the assumptions of the study.

- > This is what I have been defending from the very
- > beginning of our debate. Hence the issue of gene flow
- > versus no gene flow is only of minor relevancy for my
- > purposes.

But it is extremely important to the purpose of Bamshad et. al. That study must demonstrate that the so-called IE markers, which are fallacious all by themselves, originate from the "Aryan invasion."

The only way to that is to show evidence that the upper castes are derived almost entirely from that "invasion." And not from latter gene flow or caste creation.

- >
- > ?First of all, it studies modern DNA in South India
- > and tries to apply this to a proposed historical
- > period thousands of years ago in NW India. All of the
- > genetic samples come from some unidentified

- > "Kshatriya" caste
- > from Andhra Pradesh.?
- >
- > - Do you think that the modern DNA from NW India would
- > not show clear evidence of Y-chromosomes that are
- > ?Indo-European? markers? Indeed, did you read my
- > reference
- >

There is no such thing as an "Indo-European" marker.  
Genes are different than language or even "race."

The only thing the Bamshad et al. study shows is that  
there is some similarity in markers between two regions.

However, these same markers often turn up in higher  
ratios among non-IE Central Asians than in Europeans.

Many Rajput castes may indeed trace their lineage to  
Hun and other non-IE nomadic peoples who came to India.

- > ?Since there were many migration of NW Indians,
- > including the Moguls and their allies, to South India
- > during Muslim times, it would be interesting to note
- > the identity of this "Kshatriya" caste.
- >
- > Of course, NW India received substantial migration
- > from Central Asian during Turkic and Mogul times, so
- > any caste coming from there to South India might
- > naturally seem closer to Central Asians.?
- >
- > - Well, evidently the estimates, made by geneticists
- > by using standard methods, on the arrival times of the
- > relevant Y-chromosomes to India mean nothing to you?Or
- > might it be the case that you and your anonymous
- > specialists have never seen such estimates?
- >

That's the problem. There are no "standard methods" used  
for the "arrival times" in Bamshad et al.

They simply make their case that the arrival was connected with the Aryan invasion based on caste integrity that supposedly existed since that time.

Despite all your excessive use of quotations, you seem to have ignored that main point of my last post.

Bamshad et al. depended on a single unnamed "Kshatriya" clan to come to its conclusions.

The name of the clan is important to determine the ultimate origin of the group. Is it, for example, a Rajput group that came only in medieval times to South India?

The ancient Kshatriyas of South India were Vratyas or converts known as Dravida. That indicates that most local raja groups in South India are of local native origin.

Again, the Kshatriya caste routinely admitted powerful foreign invaders into their group. So how is it possible to determine whether any particular clan traces all or any of its heritage to a hypothetical "Aryan invasion?" Especially when the group goes unidentified.

Regards,

Paul Kekai Manansala

| 119|2002-09-28 23:03:45|a.manansala@attbi.com|Re: Cautionary Brief On the Mighty Saraswati|

- > Many Rajput castes may indeed trace their lineage to
- > Hun and other non-IE nomadic peoples who came to India.
- >

Although there have been many studies on the foreign origin of many Rajput clans, the case of the Chhetris of Nepal is documented even better.

The indigenous Magar and other lineage in the royal family and other noble Chhetri clans is well-attested in the Nepalese annals. The first members of the Gorkha royal family had Magar names and came from the Magar region.

Nearly all Chhetri clans share clan names with Magar

clans, and there are some who even still speak the Magar language.

There are also the so-called Matwali Chhetris who are of recent known mixed ancestry (Gurung, Magar, etc).

Regards,

Paul Kekai Manansala

| 120|2002-09-29 20:28:53|a.manansala@attbi.com|Fwd: Ancient weirs found at Jomon-period site |

<http://www.yomiuri.co.jp/newse/20020928wo61.htm>

Ancient weirs found at Jomon-period site

Yomiuri Shimbun

Seventeen fishing traps or weirs believed to have been placed in a river about 4,000 years ago have been discovered at an archaeological site in Ishikari, Hokkaido, the Ishikari City Board of Education said Thursday.

The site, Ishikari Momijiyama No. 49, is said to have the oldest and largest remains in the nation that indicate fishing took place in ancient rivers.

Weirs are fencelike structures for trapping fish as they swim upstream. Eight other fishing devices of a different sort also were found, proving the site to have been a major freshwater salmon fishing spot in the Jomon period (ca 10,000 B.C.-ca 300 B.C.), archeologists said.

The site is located on a dry riverbed, about 2.5 kilometers from the Ishikarigawa river.

According to the board of education, the weirs range in size from 50 centimeters by 100 centimeters to 1.4 meters by two meters. Most are rectangular and made of willow branches crossed and latticed with pieces of bark and the tendrils of wild grape vines.

Jomon period fishers probably trapped fish swimming upstream by setting the fencelike structures against stakes driven into the riverbed, a board of education official said.

Yasushi Kosugi, assistant professor at Hokkaido University, said, "We still don't know if the 17 traps were used at about the same time or used over a longer period of time, but it's a valuable find that may lead to detailed discussions on the structure and how people used it."

| 121|2002-09-29 20:31:57|a.manansala@attbi.com|Fwd: 16 ancient inscriptions, skeletons discovered in Bali |

<http://www.thejakartapost.com/detailatestnews.asp?fileid=20020929205850&irec=2>

Jakarta Post

16 ancient inscriptions, skeletons discovered in Bali

DENPASAR, Bali (JP): Head of Bali's archeological center Ayu Kusumawati disclosed here on Sunday that villagers of Tamblingan in the district of Buleleng recently found 16 ancient inscriptions containing the names of old Balinese kings.

Ayu said that the old Javanese and Balinese inscriptions were discovered about 75 centimeters beneath the trunk of a tree while the villagers renovated Ndek temple.

The copper inscription, discovered inside a porcelain artifact made 1,020 years ago, could provide historians with clues about kings who had once ruled the island, she said. Among the kings referred to on the inscription were Ungrasena, Jayapangus and Suradipa, she added.

Ayu told Antara that a team would be sent to the archaeological site next week to carry out a thorough study on it. She added it was a very noteworthy archaeological discovery because all the inscriptions were still in good condition.

Ayu also mentioned the discovery of five human skeletons, believed to be ancient Mongolid men, during an excavation at Gilimanuk, Jembrana regency, last April.

The skeletons were believed to be 2,500 years old. The team had so far collected 140 skeletons at Gilimanuk

Ancient Man Museum, about 85 kilometers west of here, since the first excavation was made in 1963, she added.

| 122|2002-10-02 08:06:46|jojomalig|Re: Me-lah-ha|

Mleccha, Milakkha or Me-lah-ha = Malacca?

obrangabalen

--- In austriac@y..., "kalyan97" wrote:

> <http://www.hindunet.org/saraswati/sindhu1.pdf>

>

> Mleccha, Milakkha or Me-lah-ha people and their language

>

> Abstract and a key to decode epigraphs

>

> The argument is that mleccha connoted the parole of a proto-Indo-

> Aryan language (or, proto-Bha\_rati\_ya language) and that mleccha

> speakers were the dominant population ? Bha\_ratam janam --, from the

> days of the R.gveda, variously referred to as da\_sa, dasyu, vra\_tya

> or asura ? all terms used as behavioral traits as were the terms,

> a\_rya or deva, connotations of 'excellence' or 'righteous

> behaviour'. There is not an iota of evidence, in early periods of

> the civilization of Bha\_ratavars.a, to treat these terms as ethnic

> identities.

>

> Art, cultural texts, epigraphs, archaeological discoveries, notes

on

> early shell- and metallurgical- techniques and economic texts are

> used to substantiate this argument.

>

> A surprise result emerges: a key to decode epigraphs using rebus

> method and proto-phonetic variants (substratum) of present-day

> languages of the Sarasvati Civilization area, relating epigraphs to

> the professions of lapidaries and smiths: shell-/stone-/mineral-

> /metal-workers.

>

> The indigenously evolved civilization matured in an extensive area

> from Ropar to Lothal along the banks of River Sarasvati and the

> cultural heritage lives on in Bha\_rata, thanks to the contributions

> made by mleccha (me-lah-ha), contributions exemplified by samudra

> manthanam (churning of the ocean), as a co-operative endeavour

among

> bha\_ratam janam.

| 123|2002-10-02 08:38:56|jojomalig|Re: Water buffalo domestication|

--- In austriac@y..., "pinatubo.geo" wrote:

> We hypothesise that the species

> originated in mainland south-east Asia, and that it spread north to



> China and west to the Indian subcontinent, where the river type

evolved

> and was domesticated. Following domestication in China, the

> domesticated swamp buffalo spread through two separate routes,

through

> Taiwan and the Philippines to the eastern islands of Borneo and

> Sulawesi, and south through mainland south-east Asia and then to the

> western islands of Indonesia.

A point of clarity. The hypothesized route follows the Bellwood Austronesian dispersion theory pattern.

Jojo Malig

| 124|2002-10-02 08:57:14|jojomalig|Archeoastronomy|

This link is worth visiting.

<http://www.wam.umd.edu/~tlaloc/archastro/journals.html>

Jojo Malig

| 125|2002-10-02 09:15:16|jojomalig|Pre-contact religious sites|

Is it correct to surmise that the lack (non-discovery) of pre-contact temple sites in the Philippines (as compared to Borobodur) was that the temples were made of wood and not stone?

Jojo Malig

| 126|2002-10-02 17:15:07|a.manansala@attbi.com|Re: Water buffalo domestication|

> --- In austric@y..., "pinatubo.geo" wrote:

> > We hypothesise that the species

> > originated in mainland south-east Asia, and that it spread north to

> > China and west to the Indian subcontinent, where the river type

> evolved

> > and was domesticated. Following domestication in China, the

> > domesticated swamp buffalo spread through two separate routes,

> through

> > Taiwan and the Philippines to the eastern islands of Borneo and

> > Sulawesi, and south through mainland south-east Asia and then to the

> > western islands of Indonesia.

>

> A point of clarity. The hypothesized route follows the Bellwood

> Austronesian dispersion theory pattern.

>

With the exception of course of the Pacific island migrations.

The most common explanation, if we accept that Proto-Austronesians used the domesticated water buffalo, is that the first migrations reached islands where the water buffalo was not practical. Or, alternately, environments like atolls where the water buffalo could not survive.

Regards,

Paul Kekai Manansala

| 127|2002-10-05 01:14:32|TTT UUU|Re: Pre-contact religious sites|

Stephen Oppenheimer's book Eden in the East, suggests that many early (temples? and) living sites were sunmerged 7-8 thou. years ago when the ice masses over Canada finally melted and released blocked meltwater. Much of lowland Philipines would have been submerged.

There is a drowned megalithic structure to the east of Taiwan. It has been popularized by Japanese divers. Perhaps they discovered it.

Robin Day

*jojomalig* wrote:

Is it correct to surmise that the lack (non-discovery) of pre-contact temple sites in the Philippines (as compared to Borobodur) was that the temples were made of wood and not stone?

Jojo Malig

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| 128|2002-10-05 03:05:57|Jojo Malig|Re: Atlantis in Asia|

Robin,

I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia" theory - or the existence of a highly relatively advanced civilization in South East Asia. I would agree with the notion that structures built along seashores (or low terrain) would have been submerged after the great melting of ice lands in Canada. But how about those in mountain areas? such as Cordillera, perhaps? If there were stone-built pre-contact religious temple sites there, they wouldn't be affected by rising waters.

regards,

Jojo Malig

*austriac@yahoogroups.com* wrote:

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Topics in this digest:

1. Re: Pre-contact religious sites  
From: TTT UUU

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-----  
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Message: 1  
Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)  
From: TTT UUU  
Subject: Re: Pre-contact religious sites

Stephen Oppenheimer's book Eden in the East, suggests that many early (temples? and) living sites were sunmerged 7-8 thou. years ago when the ice masses over Canada finally melted and released blocked meltwater. Much of lowland Philipines would have been submerged. There is a drowned megalithic structure to the east of Taiwan. It has been popularized by Japanese divers. Perhaps they discovered it.

Robin Day

jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of pre-contact temple sites in the Philipines (as compared to Borobodur) was that the temples were made of wood and not stone?

Jojo Malig

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- Oscar Wilde

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| 129|2002-10-05 13:40:24|jojomalig|CALL FOR PAPERS: 2002 Philippine Linguistics Conference|

To: [Alibata@yahoogroups.com](mailto:Alibata@yahoogroups.com)

From: "philippinestudies" <[suligi22@aol.com](mailto:suligi22@aol.com)>

Date: Sat, 05 Oct 2002 19:27:30 -0000

Subject: [Alibata] CALL FOR PAPERS

## CALL FOR PAPERS

The Linguistic Society of the Philippines (LSP) announces an International Conference on the theme

"Applied Linguistics and Language Education: Theory and Practice"

## DATES AND VENUE

Manila Midtown Hotel

December 9-11, 2002

## OBJECTIVES

bring together scholars, consultants and leaders in the fields of linguistics and applied linguistics, national and international, for an active interchange of ideas, experiences and insights to achieve synergistic and relevant language education programs for the new

millennium;  
relate knowledge about linguistics and applied linguistics to the curriculum, learners, teachers, instructional materials, methods and techniques as well as evaluation in the language teaching/learning process;  
strengthen the participants' knowledge and perception of how various factors in the environment affect language and communications;  
discuss recent developments and develop strategies in understanding and managing change in the global arena;  
present studies and research findings in linguistics and applied linguistics in multicultural settings for charting directions in policy decision making and future research to meet the challenges brought about by the emerging new technologies.

#### TOPICS

bilingual/multilingual/multicultural education  
content-area instruction  
discourse analysis  
language teaching  
language testing  
literacy (basic and functional)  
pragmatics  
psycholinguistics  
sociolinguistics

#### DEADLINE AND CONTACT DETAILS

Abstracts and bio-data may be sent to the following email addresses on or before March 31, 2002:

[opmov@mail.dlsu.edu.ph](mailto:opmov@mail.dlsu.edu.ph)

[emmasc@hotmail.com](mailto:emmasc@hotmail.com)

Isabel Pefianco Martin, Ph.D.  
Chair, Department of English  
School of Humanities  
Ateneo de Manila University  
Loyola Heights 1108,  
Quezon City, Philippines  
Telefax (632)426-6120

| 130|2002-10-06 18:47:52|TTT UUU|Re: Philippine?/Okinawa Stone buildings|

I know little of Philippine archaeology. Do you refer to the Philippine mountains as Cordillera (Spanish?). In Canada/Europe post holes indicate the presence of old buildings, now all rotted away. Perhaps the same is true in the Philippines. I know of no stone structures for the Philippines early history.

Update: The submerged stone buildings east of Taiwan were actually in the Okinawa island chain(Japanese), Yonaguni Island for example (Oppenheimer photo 33), 25 meters below the sea. I will do a net search on these words as I have only heard a littlebit about them.

I have little to add about your observation of no stone buildings in the Philipinesuplands.Upland caves have provided useful info in Borneo and the Malaya Peninsula.Perhaps the Philipines were simplyleft out of many of the cultural comings and goings. Why? Hostile natives? Again, a significant site may be submerged and yet to be discovered.

Robin Day, S. Korea

**Jojo Malig** wrote:

Robin,

I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia" theory - or the existence of a highly relatively advanced civilization in South East Asia. I would agree with the notion that structures built along seashores (or low terrain) would have been submerged after the great melting of ice lands in Canada. But how about those in mountain areas? such as Cordillera, perhaps? If there were stone-built pre-contact religious temple sites there, they wouldn't be affected by rising waters.

regards,

Jojo Malig

**austrie@yahoo.com** wrote:

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Topics in this digest:

1. Re: Pre-contact religious sites

From: TTT UUU

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Message: 1

Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)

From: TTT UUU

Subject: Re: Pre-contact religious sites

Stephen Oppenheimer's book Eden in the East, suggests that many early (temples? and) living sites were sunmerged 7-8 thou. years ago when the ice masses over Canada finally melted and released blocked meltwater. Much of lowland Philipines would have been submerged.

There is a drowned megalithic structure to the east of Taiwan. It has been popularized by Japanese divers. Perhaps they discovered it.

Robin Day

jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of pre-contact temple sites in the Philippines (as compared to Borobodur) was that the temples were made of wood and not stone?

Jojo Malig

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| 131|2002-10-06 18:59:01|TTT UUU|Re: Maps of the Ancient Sea Kings (Hapgood)|

I should have mentioned this book was written by American geography Professor Charles Hapgood. His comments on the ancient dating of the earthen conical pyramid at Cuicuillo, Mexico, and the megalithic centre near Lake Titicaca, Bolivia, are the small areas he should have steered clear of. He had no direct experience here and he cited studies published prior to radiocarbon dating. The Cuicuillo pyramid is not ancient, as suggested, and a good review appears on the web. Stay tuned.

*gymnogoy* wrote:

This scholarly book (purchased through Amazon.com) was reprinted in the 90s and it has opened my mind. Much of the ancient world was accurately mapped long before Columbus. In fact Columbus may have obtained a copy of one of the ancient maps. These maps are believed to be the oldest human writings.

Robin Day Korea

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| 132|2002-10-06 19:50:57|a.manansala@attbi.com|Re: Philippine?/Okinawa Stone buildings|

>

> I know little of Philippine archaeology. Do you refer to the Philippine mountains  
> as Cordillera (Spanish?). In Canada/Europe post holes indicate the presence of  
> old buildings, now all rotted away. Perhaps the same is true in the Philippines.  
> I know of no stone structures for the Philippines early history.

There are a few although only very little has been written about most of them.

One is the base of what apparently was a stone pyramid in Benguet province. There are also the remains of a massive "fortress" wall that has been dated to 4000 BP in the northeastern corner of Ifugao province in Potia municipality.

Of course, there are also the stone terrace walls of Bontoc that have been dated widely from geological datings up to 11,000 BP, radiocarbon datings at 3000 BP and other datings to post-Hispanic times.

All the above in the Philippine Cordillera.

There are also some stone terrace walls else where and the stone baths of Bohol, but I don't know recall if any dates have been ascertained for these.

In the Batanes islands, stone "fortresses" known as "idyang" and similar to the Okinawan gusuku. If I remember correctly the earliest of these date to 2000 BP. The people in this are traditionally have built stone houses with thatch roofs.

Regards,  
Paul Kekai Manansala

- > Update: The submerged stone buildings east of Taiwan were actually in the
- > Okinawa island chain (Japanese), Yonaguni Island for example (Oppenheimer photo
- > 33), 25 meters below the sea. I will do a net search on these words as I have
- > only heard a little bit about them.
- > I have little to add about your observation of no stone buildings in the
- > Philipines uplands. Upland caves have provided useful info in Borneo and the
- > Malaya Peninsula. Perhaps the Philipines were simply left out of many of the
- > cultural comings and goings. Why? Hostile natives? Again, a significant site may
- > be submerged and yet to be discovered.
- > Robin Day, S. Korea
- > Jojo Malig <[jojomalig@yahoo.com](mailto:jojomalig@yahoo.com)> wrote:
- > Robin,
- > I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia"
- > theory - or the existence of a highly relatively advanced civilization in South
- > East Asia. I would agree with the notion that structures built along seashores
- > (or low terrain) would have been submerged after the great melting of ice lands

> in Canada. But how about those in mountain areas? such as Cordillera, perhaps?  
> If there were stone-built pre-contact religious temple sites there, they  
> wouldn't be affected by rising waters.  
> regards,  
> Jojo Malig

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> Topics in this digest:  
>  
> 1. Re: Pre-contact religious sites  
> From: TTT UUU

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> \_\_\_\_\_  
> \_\_\_\_\_  
>  
> Message: 1  
> Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)  
> From: TTT UUU  
> Subject: Re: Pre-contact religious sites

>  
>  
> Stephen Oppenheimer's book Eden in the East, suggests that many early (temples?  
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> Robin Day  
> jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of  
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> temple sites in the Philippines (as compared to Borobodur) was that  
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>  
> Jojo Malig  
>  
>  
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> quotation." - Oscar Wilde

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| 133|2002-10-07 05:39:07|Jojo Malig|Re: pre-contact structures|

Robin, Paul,

Thanks for all the information. By the way, as for the other areas in the archipelago, it is a theory that the majority of pre-contact religious sites were made of wood and not stone, as compared to other pre European contact civilizations in South America and Southeast Asia.

Wood edifices, though far easier to build, wouldn't be as permanent as stone edifices, and Robin is correct that holes for foundation posts are clues for areas where pre-contact religious sites could have been built.

As for the Bontoc, Cordillera and Batanes structures, thanks Paul for the info.

regards,

Jojo Malig

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- Oscar Wilde

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| 134|2002-10-07 12:22:17|a.manansala@attbi.com|Re: pre-contact structures|

>  
> Robin, Paul,

- >
- > Thanks for all the information. By the way, as for the other areas in the
- > archipelago, it is a theory that the majority of pre-contact religious sites were
- > made of wood and not stone, as compared to other pre-European contact
- > civilizations in South America and Southeast Asia.
- >

The early-contact European writings confirm that most of the large buildings in the Philippines and Borneo were constructed of wood.

Regards,  
 Paul Kekai Manansala  
 | 135|2002-10-08 19:59:09|TTT UUU|Re: pre-contact structures|

Thanks Paul: Wood construction is/was also predominant in New Guinea as well.

### **Mexican Links**

I am just finishing Oppenheimer's book (on my lap now) and remembered the "tree of life" motif and mythic stories were very prominent among the Maya. It has the same elements: the tree axis (often a cross) with foliage and fruits sometimes, a celestial bird at the top and a crocodile at the base and a serpent in the branches. This motif is often worked in stone or plaster at temple sites. I have several books with these illustrations and if I remember correctly the most famous would be King Pacal's stone tomb lid at Palenque. This to me is no coincidence. Austronesians must have crossed the Pacific and introduced some of their technology. There was no bronze work among the early Maya/Mexicans but it appeared later among the Mexica in a limited area so presumably this technology was not transferred at an early period. [The Inca also had bronze for ceremonial use among the court.] The Mexicans also made paper bark cloth also from Mulberry, and I have commented earlier that sweet potato was probably carried back across the Pacific from S. America in canoe by the Polynesians, or earlier Austronesians.

Oppenheimer (photo 26 & 27) points out bronze (+ pottery base) money trees in Sichuan Chinese burials and these had birds in the branches and a monster at the base. Surprise! Similar figures/sculptures were excavated in the Paekjae kingdom of west Korea. A very famous bronze lidded incense burner is crafted in this fashion also with a dragon at the base. Should be easy to find this illustration with a web search. The bronze mask photo 25 also found in Sichuan is almost identical to an object (flag pole cap) from the same Korean Paekjae culture. In China this mask is known as the T'ao-t'iet or Glutton. It is associated with human sacrifice. The lower jaw is often not illustrated. The story goes that the glutton often associated with human death from drought lost its lower jaw as a punishment. The Glutton mask is commonly illustrated on old Chinese and Vietnamese bronze dynastic cauldrons. These cauldrons held wine or blood at different times. As time passed the mask became more abstract and is harder to recognize. Later versions have a long undulating nose and look so much like the Yucatan Chac god masks seen all over some Mayan temples (eg. Labna, Chichen Itza, etc.). Another surprise: these Mayan examples have no jaw and

also bear the undulating nose. In Mexico the Chac god was appealed to for rain also associated with drought and human death. In northern areas of Mexico Chac is known by the name of Tlaloc. I guess we have the working hypothesis that the Chinese T'ao-tieh is in fact the Mexican Tlaloc/Chac and was introduced to Mexico's Pacific coast by Austronesians, master mariners.

Will cite some of these illustrated books I have later.

Regards

Robin Day S. Korea

*a.manansala@attbi.com* wrote:

>  
> Robin, Paul,  
>  
> Thanks for all the information. By the way, as for the other areas in  
teh  
> archipelago, it is a theory that the majority of pre-contact religious sites  
were  
> made of wood and not stone, as compared to other pre European contact  
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Regards,  
Paul Kekai Manansala

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| 136|2002-10-08 23:32:55|TTT UUU|Re: Eden/Atlantis in SE Asia|

Wondering where I can find out more about pre Hindu/Buddhist stone building in Indonesia. I would like to get some popular books to start, something that is in print. I remember a documentary, maybe National Geographic where an Indonesian man paid a huge sum in food to guests to have a huge stone hauled overland by people so that he could honour his dead father. The stone may have been a memorial capstone.

We have a special stone-working tradition in Korea, on the south island of Cheju. The native Tamna people carved tall "grandfather" statues in volcanic rock. they look like small Easter Island Moai. This tradition may have links with south Asian and Micronesia (Guam, Okinawa, Polynesia etc.). The people of Cheju still speak a strange dialect and I suspect it is the remnant of a language distinct from Korean. Korea is today composed mainly of peoples who migrated from the north, through Manchuria. Many were associated with a horse culture, eg. the Puyo. Cheju Island also has a few people who exhibit the curly or kinky hair associated with some Austric peoples. This is a rare trait now but I have met one man from Cheju who lives locally in Busan.

**Jojo Malig** wrote:

Robin,

I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia" theory - or the existence of a highly relatively advanced civilization in South East Asia. I would agree with the notion that structures built along seashores (or low terrain) would have been submerged after the great melting of ice lands in Canada. But how about those in mountain areas? such as Cordillera, perhaps? If there were stone-built pre-contact religious temple sites there, they wouldn't be affected by rising waters.

regards,

Jojo Malig

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Topics in this digest:

1. Re: Pre-contact religious sites  
From: TTT UUU

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Message: 1  
Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)  
From: TTT UUU  
Subject: Re: Pre-contact religious sites

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There is a drowned megalithic structure to the east of Taiwan. It has been popularized by Japanese divers. Perhaps they discovered it.

Robin Day

jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of pre-contact temple sites in the Philippines (as compared to Borobodur) was that the temples were made of wood and not stone?

Jojo Malig

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| 137|2002-10-09 04:46:33|Celia Ehrlich|TOL, dragon and Cordyline fruticosa|

In my dissertation (Ehrlich 1999), I found that places simulating a "tree of life" with a celestial bird at the top, a crocodile at the base and/or a serpent in the branches often had ti plants growing nearby. I began to expect to see this species, Cordyline fruticosa, at such places, places where people tried through ceremonies to influence powerful supernaturals. This was true of the Ngaju, the Kenyah, the Iban in Borneo, the Toraja in Sulawesi, the Karo Batak in Sumatra, the Ifugao in Luzon, many groups in Papua New Guinea and Island Melanesia and, of course, in Polynesia.

The journal Anthropos recently rejected an article I wrote, arguing that the most probable reason for the phenomenon was that the idea of a reddish plant as a sort of cinnabar, was that people had continued to believe they could influence supernaturals by a kind of alchemy. Isn't this more likely than that the associations between tree of life, dragon-like supernaturals and ti plants should have been re-invented repeatedly?

Celia Ehrlich

-----  
Celia Ehrlich  
254 Poverty Lane  
Lebanon, NH 03766-2702  
[cehrlich@cyberportal.net](mailto:cehrlich@cyberportal.net)

| 138|2002-10-09 08:34:21|a.manansala@attbi.com|Re: Eden/Atlantis in SE Asia|

There is a theory that the base ziggurats at Borodudor and Angkor Wat are from the pre Hindu/Buddhist period. The stone work is different at the core of both structures. Unfortunately, I'm too busy right now to track down the refs. Many small early stepped pyramid-like have been found throughout SE Asia.

The early stone structures of Indonesia tend to resemble those in Pacific. A lot of megaliths with a

bit of stone carving here and there with terraced architecture.

Regards,  
Paul Kekai Manansala

>  
> Wondering where I can find out more about pre Hindu/Buddhist stone building in  
> Indonesia. I would like to get some popular books to start, something that is in  
> print. I remember a documentary, maybe National Geographic where an Indonesian  
> man paid a huge sum in food to guests to have a huge stone hauled overland by  
> people so that he could honour his dead father. The stone may have been a  
> memorial capstone.  
> We have a special stone-working tradition in Korea, on the south island of  
> Cheju. The native Tamna people carved tall "grandfather" statues in volcanic  
> rock. they look like small Easter Island Moai. This tradition may have links  
> with south Asian and Micronesia (Guam, Okinawa, Polynesia etc.) . The people of  
> Cheju still speak a strange dialect and I suspect it is the remnant of a language  
> distinct from Korean. Korea is today composed mainly of peoples who migrated  
> from the north, through Manchuria. Many were associated with a horse culture, > eg. the Puyo.  
Cheju Island also has a few people who exhibit the curly or kinky  
> hair associated with some Austric peoples. This is a rare trait now but I have  
> met one man from Cheju who lives locally in Busan.  
> Jojo Malig <[jojomalig@yahoo.com](mailto:jojomalig@yahoo.com)> wrote:  
> Robin,  
> I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia"  
> theory - or the existence of a highly relatively advanced civilization in South  
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> If there were stone-built pre-contact religious temple sites there, they  
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> regards,  
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| 139|2002-10-10 00:28:29|TTT UUU|Re: TOL, dragon and Cordyline fruticosa|

Hi Celia

I'm a botanist too, temperate and arctic plants mostly.

I don't entirely follow your last note because of a few of the words you are using, like alchemy and cinnabar.

Are you suggesting the red Cordyline was planted near religious sites as a form of sympathetic magic/influence?

I'd like to read your hypothesis in more detail.

Try to get your manuscript published elsewhere or revise it and discuss it with us here.

Robin Day S. Korea

**Celia Ehrlich** wrote:

In my dissertation (Ehrlich 1999), I found that places simulating a "tree of life" with a celestial bird at the top, a crocodile at the base and/or a serpent in the branches often had ti plants growing nearby. I began to expect to see this species, *Cordyline fruticosa*, at such places, places where people tried through ceremonies to influence powerful supernaturals. This was true of the Ngaju, the Kenyah, the Iban in Borneo, the Toraja in Sulawesi, the Karo Batak in Sumatra, the Ifugao in Luzon, many groups in Papua New Guinea and Island Melanesia and, of course, in Polynesia.

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| 140|2002-10-10 01:09:29|TTT UUU|Re: Philipine?/Okinawa Stone buildings|

Thanks Paul

I don't know what an Okinawa "gusuku" looks like but the Cheju Island people( S. Korea)also make their oval or rectangular one-storyhouses with unfinishedvolcanic stones, chinked with clay andcovered with a thatch roof.

After sending my last inquiry to this websiteI didremember the amazing (stone)walls of the ricefield terraces in the Philipine uplands and you suggest their dating is uncertain.Thanks. Will search Amazon to see what books I can find.

Robin Day S. Korea

*a.manansala@attbi.com* wrote:

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- > I know litte of Philipine archaeology. Do you refer to the Philipine mountains
- > as Cordillera (Spanish?). In Canada/Europe post holes indicate the presence of
- > old buildings, now all rotted away. Perhaps the same is true in the Philipines.
- > I know of no stone structures for the Philipines early history.

There are a few although only very little has been written about most of them.

One is the base of what apparently was a stone pyramid in Benguet province. There are also the remains of a massive "fortress" wall that has been dated to 4000 BP in the northeastern corner of Ifugao province in Potia municipality.

Of course, there are also the stone terrace walls of Bontoc that have been dated widely from geological

datings up to 11,000 BP, radiocarbon datings at 3000 BP and other datings to post-Hispanic times.

All the above in the Philippine Cordillera.

There are also some stone terrace walls else where and the stone baths of Bohol, but I don't know recall if any dates have been ascertained for these.

In the Batanes islands, stone "fortresses" known as "idyang" and similar to the Okinawan gusuku. If I remember correctly the earliest of these date to 2000 BP. The people in this are traditionally have built stone houses with thatch roofs.

Regards,  
Paul Kekai Manansala

> Update: The submerged stone buildings east of Taiwan were actually in the  
> Okinawa island chain (Japanese), Yonaguni Island for example (Oppenheimer photo  
> 33), 25 meters below the sea. I will do a net search on these words as I have  
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| 141|2002-10-10 04:32:10|Celia Ehrlich|Re: TOL, dragon and Cordyline fruticosa|

On Thu, 10 Oct 2002 03:28:28 -0400 (EDT), TTT UUU wrote:  
TTT responded to my recent message, puzzled by my use of  
words like "alchemy" and "cinnabar." 'Hoping more of  
you will become interested in the problem, I am sending  
my answer to her to all of you:

Hi Robin,

I'm interested but not surprised to find that "alchemy" and "cinnabar" put you off. After I learned about ti as a sacred plant, I began thinking about the difference between the Middle East, especially Socotra where *Dracaena cinnabari* had been discovered by Balfour in the 1880's, and Island Southeast Asia, where these trees did not produce the red "cinnabari" or "dragon's blood" that was an important article of trade across the Indian Ocean to Sumatra until about 700 AD. Schoff's rendition of the Periplus of the Erythraean Sea tells about the early trade. After that, the business went the other way with "dragon's blood" from various rattans and other Malaysian plants. "Dragon's blood" was "cinnabari" because people used it as a panacea, trying to restore health in a process equivalent to alchemy. Pliny the elder noticed the parallel (book xxxiii, pp. 114-117 in Rackham and Jones' version).

So I thought, the simplest explanation of the widespread use of reddish ti plants at altars would be that the plant was a substitute for "dragon's blood." I had noticed the crocodile carvings and other allusions to dragonlike creatures in ceremonies at such altars. The whole was much like the Tree in the Garden of Eden. The simplest explanation would be that the idea of transformation by "dragon's blood" had continued even when the original African and Socotran material was no longer available. We know that Chinese hsien-men used vegetable as well as mineral "cinnabar" and I'd found a Japanese name for the ti plant, \*sennenboku\* which an expert at Dartmouth told me could be shaman-tree. This is the idea I developed in the unpublished paper, "Proxies for Cinnabar: Plants in an "Alchemy of Life."

Thanks for responding to my e-mail. I think I'd better send this explanation to the group. It is complicated.

Celia Ehrlich

[cehrlich@cyberportal.net](mailto:cehrlich@cyberportal.net)

>

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| 142|2002-10-10 08:51:11|a.manansala@attbi.com|Re: Philippine?/Okinawa Stone buildings|  
Here are a few photo links of gusuku:

<http://www.marusiba.co.jp/gusiku/gusuku3.JPG>  
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[www.asahi-net.or.jp/~nj3t-wtnb/aria10/photo/zakimi.gif](http://www.asahi-net.or.jp/~nj3t-wtnb/aria10/photo/zakimi.gif)  
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| 143|2002-10-10 19:36:42|TTT UUU|Re: Indonesia/Okinawa Stone buildings|

Paul those Okinawa "gusuku" do look like fortresses. Thanks.

Reading last night in Oppenheimer's book that the people of Flores, Tamimbar Island, and the Batak of Sumatra have a megalithic tradition.

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Here are a few photo links of gusuku:

<http://www.marusiba.co.jp/gusuku/gusuku3.JPG>  
[http://www.bihou.com/iseki/gusuku/gusuku\\_001.jpg](http://www.bihou.com/iseki/gusuku/gusuku_001.jpg)  
[www.asahi-net.or.jp/~nj3t-wtnb/aria10/photo/zakimi.gif](http://www.asahi-net.or.jp/~nj3t-wtnb/aria10/photo/zakimi.gif)  
[http://museum.mm.pref.okinawa.jp/web\\_e/history/photo/photo\\_h/aaa0163b.jpg](http://museum.mm.pref.okinawa.jp/web_e/history/photo/photo_h/aaa0163b.jpg)

Regards,  
Paul Kekai Manansala

>  
> Thanks Paul  
> I don't know what an Okinawa "gusuku" looks like but the Cheju Island  
> people ( S. Korea) also make their oval or rectangular one-story houses  
with  
> unfinished volcanic stones, chinked with clay and covered with a thatch  
roof.  
> After sending my last inquiry to this website I did remember the  
amazing (stone)  
> walls of the rice field terraces in the Philippine uplands and you suggest  
their  
> dating is uncertain. Thanks. Will search Amazon to see what books I  
can find.  
> Robin Day S. Korea  
> a.manansala@attbi.com wrote:

> >

> > I know little of Philippine archaeology. Do you refer to the Philippine mountains

> > as Cordillera (Spanish?). In Canada/Europe post holes indicate the presence of

> > old buildings, now all rotted away. Perhaps the same is true in the Philippines.

> > I know of no stone structures for the Philippines early history.

>

>

> There are a few although only very little has been written about most of them.

>

> One is the base of what apparently was a stone pyramid

> in Benguet province. There are also the remains of a

> massive "fortress" wall that has been dated to 4000 BP

> in the northeastern corner of Ifugao province in Potia

> municipality.

>

> Of course, there are also the stone terrace walls of

> Bontoc that have been dated widely from geological

> datings up to 11,000 BP, radiocarbon datings at 3000 BP

> and other datings to post-Hispanic times.

>

> All the above in the Philippine Cordillera.

>

> There are also some stone terrace walls elsewhere and

> the stone baths of Bohol, but I don't know recall if any

> dates have been ascertained for these.

>

> In the Batanes islands, stone "fortresses" known as

> "idyang" and similar to the Okinawan gusuku. If

> I remember correctly the earliest of these date to

> 2000 BP. The people in this area traditionally have built

> stone houses with thatch roofs.

>

> Regards,

> Paul Kekai Manansala

>

>

>

> > Update: The submerged stone buildings east of Taiwan were actually in the

> > Okinawa island chain (Japanese), Yonaguni Island for example (Oppenheimer

> photo

> > 33), 25 meters below the sea. I will do a net search on these words as I have  
> > only heard a little bit about them.  
> > I have little to add about your observation of no stone buildings in the  
> > Philipines uplands. Upland caves have provided useful info in Borneo and the  
> > Malaya Peninsula. Perhaps the Philipines were simply left out of many of the  
> > cultural comings and goings. Why? Hostile natives? Again, a significant site  
> may  
> > be submerged and yet to be discovered.  
> > Robin Day, S. Korea  
> > Jojo Malig wrote:  
> > Robin,  
> > I have heard about Oppenheimer's work, which puts forth the "Atlantis in  
> Asia"  
> > theory - or the existence of a highly relatively advanced civilization in  
> South  
> > East Asia. I would agree with the notion that structures built along seashores  
> > (or low terrain) would have been submerged after the great melting of ice  
> lands  
> > in Canada. But how about those in mountain areas? such as Cordillera, perhaps?  
> > If there were stone-built pre-contact religious temple sites there, they  
> > wouldn't be affected by rising waters.  
> > regards,  
> > Jojo Malig  
> >  
> >  
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> >  
> >  
> > -----  
> >  
> > There is 1 message in this issue.  
> >  
> > Topics in this digest:  
> >  
> > 1. Re: Pre-contact religious sites  
> > From: TTT UUU

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>> Message: 1  
>> Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)  
>> From: TTT UUU  
>> Subject: Re: Pre-contact religious sites

>>

>>

>> Stephen Oppenheimer's book Eden in the East, suggests that many early

> (temples?

>> and) living sites were sunmerged 7-8 thou. years ago when the ice masses over

>> Canada finally melted and released blocked meltwater. Much of lowland

> Philipines

>> would have been submerged.

>> There is a drowned megalithic structure to the east of Taiwan. It has been

>> popularized by Japanese divers. Perhaps they discovered it.

>> Robin Day

>> jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of

>> pre-contact

>> temple sites in the Philippines (as compared to Borobodur) was that

>> the temples were made of wood and not stone?

>>

>> Jojo Malig

>>

>>

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>> "Most people are other people - their lives a mimicry, their passions,

a

>> quotation." - Oscar Wilde

>>

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| 144|2002-10-10 19:45:10|TTT UUU|Re: pre-contact structures|

The book I found so useful on Chinese art/mythology:

Christie Anthony. 1968. Chinese Mythology Paul Hamlyn Press. Has several illustrations of the T'ao-tieh(Glutton) masks, usually in bronze.



**TTT UUU** wrote:

Thanks Paul: Wood construction is/was also predominant in New Guinea as well.

### **Mexican Links**

I am just finishing Oppenheimer's book (on my lap now) and remembered the "tree of life" motif and mythic stories was very prominent among the Maya. It has the same elements: the tree axis (often a cross) with foliage and fruits sometimes, a celestial bird at the top and a crocodile at the base and a serpent in the branches. This motif is often worked in stone or plaster at temple sites. I have several books with these illustrations and if I remember correctly the most famous would be King Pacal's stone tomb lid at Palenque. This to me is no coincidence. Austronesians must have crossed the Pacific and introduced some of their technology. There was no bronze work among the early Maya/Mexicans but it appeared later among the Mexic in a limited area so presumably this technology was not transferred at an early period. [The Inca also had bronze for ceremonial use among the court.] The Mexicans also made paper bark cloth also from Mulberry, and I have commented earlier that sweet potato was probably carried back across the Pacific from S. America in canoe by the Polynesians, or earlier Austonesians.

Oppenheimer (photo 26 & 27) points out bronze ( + pottery base) money trees in Sichuan Chinese burials and these had birds in the branches and a monster at the base. Surprise! Similar figures/sculptures were excavated in the Paekjae kingdom of west Korea. A very famous bronze lidded incense burner is crafted in this fashion also with a dragon at the base. Should be easy to find this illustration with a web search. The bronze mask photo 25 also found in Sichuan is almost identical to an object (flag pole cap) from the same Korean Paekjae culture. In China this mask is known as the T'ao-t'iet or Glutton. It is associated with human sacrifice. The lower jaw is often not illustrated. The story goes that the glutton often associated with human death from drought lost its lower jaw as a punishment. The Glutton mask is commonly illustrated on old Chinese and Vietnamese bronze dynastic cauldrons. These cauldrons held wine or blood at different times. As time passed the mask became more abstract and is harder to recognize. Later versions have a long undulating nose and look so much like the Yucatan Chac god masks seen all over some Mayan temples (eg. Labna, Chichen Itza, etc.). Another surprise: these Mayan examples have no jaw and also bear the undulating nose. In Mexico the Chac god was appealed to for rain also associated with drought and human death. In northern areas of Mexico Chac is known by the name of Tlaloc. I guess we have the working hypothesis that the

ChineseT'ao-tieh is in fact the MexicanTlaloc/Chac and was introduced to Mexico's Pacific coastby Austronesians, master mariners.

Will cite some of these illustratedbooks I have later.

Regards

Robin Day S. Korea

*a.manansala@attbi.com* wrote:

>  
> Robin, Paul,  
>  
> Thanks for all the information. By the way, as teh for the other  
areas in teh  
> archipelago, it is a theory that the majority of pre-contact  
religious sites were  
> made of wood and not stone, as compared to other pre European  
contact  
> civilizations in South America and Southeast Asia.  
>

The early-contact European writings confirm that most of the  
large buildings in the Philippines and Borneo were  
constructed of wood.

Regards,  
Paul Kekai Manansala

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| 145|2002-10-10 20:37:01|TTT UUU|Re: Eden/Atlantis in SE Asia|

I had luck finding photos with a search of Borobudur, Java.

*a.manansala@attbi.com* wrote:

There is a theory that the base ziggurats at Borobudur and Angkor Wat are from the pre Hindu/Buddhist period. The stone work is different at the core of both structures. Unfortunately, I'm too busy right now to track down the refs. Many small early stepped pyramid-like have been found throughout SE Asia.

The early stone structures of Indonesia tend to resemble those in Pacific. A lot of megaliths with a bit of stone carving here and there with terraced architecture.

Regards,  
Paul Kekai Manansala

>

> Wondering where I can find out more about pre Hindu/Buddhist stone building in

> Indonesia. I would like to get some popular books to start, something that is in

> print. I remember a documentary, maybe National Geographic where an Indonesian

> man paid a huge sum in food to guests to have a huge stone hauled overland by

> people so that he could honour his dead father. The stone may have been a  
> memorial capstone.  
> We have a special stone-working tradition in Korea, on the south island of  
> Cheju. The native Tamna people carved tall "grandfather" statues in volcanic  
> rock. they look like small Easter Island Moai. This tradition may have links  
> with south Asian and Micronesia (Guam, Okinawa, Polynesia etc.) .  
The people of  
> Cheju still speak a strange dialect and I suspect it is the remnant of a language  
> distinct from Korean. Korea is today composed mainly of peoples who migrated  
> from the north, through Manchuria. Many were associated with a horse culture, > eg. the Puyo. Cheju Island also has a few people who exhibit the curly or kinky  
> hair associated with some Austric peoples. This is a rare trait now but I have  
> met one man from Cheju who lives locally in Busan.  
> Jojo Malig wrote:  
> Robin,  
> I have heard about Oppenheimer's work, which puts forth the "Atlantis in Asia"  
> theory - or the existence of a highly relatively advanced civilization in South  
> East Asia. I would agree with the notion that structures built along seashores  
> (or low terrain) would have been submerged after the great melting of ice lands  
> in Canada. But how about those in mountain areas? such as Cordillera, perhaps?  
> If there were stone-built pre-contact religious temple sites there, they  
> wouldn't be affected by rising waters.  
> regards,  
> Jojo Malig  
>  
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> 1. Re: Pre-contact religious sites  
> From: TTT UUU  
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> Message: 1  
> Date: Sat, 5 Oct 2002 04:14:28 -0400 (EDT)  
> From: TTT UUU  
> Subject: Re: Pre-contact religious sites  
>  
>  
> Stephen Oppenheimer's book Eden in the East, suggests that many early  
> (temples?  
> and) living sites were sunmerged 7-8 thou. years ago when the ice  
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> Canada finally melted and released blocked meltwater. Much of  
> lowland Philipines  
> would have been submerged.  
> There is a drowned megalithic structure to the east of Taiwan. It has  
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> popularized by Japanese divers. Perhaps they discovered it.  
> Robin Day  
> jojomalig wrote: Is it correct to surmise that the lack (non-discovery) of  
> pre-contact  
> temple sites in the Philippines (as compared to Borobodur) was that  
> the temples were made of wood and not stone?  
>  
> Jojo Malig  
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> "Most people are other people - their lives a mimicry, their passions, a  
> quotation." - Oscar Wilde

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| 146|2002-10-11 09:20:41|Paul Kekai Manansala|Re: TOL, dragon and Cordyline fruticosa|  
Celia, I remember you connected this plant earlier with the East  
African trade mentioned by Schoff.

If the motifs are found out in the Pacific does that suggest that  
the transfer of the plant took place before the Lapita expansion?

I may have missed it but does the idea of a reddish cinnabar  
associated with alchemy, dragon-like creatures and the  
tree of life also occur in the East African context?

Regards,  
Paul Kekai Manansala

--- In [austrie@y...](mailto:austrie@y...), "Celia Ehrlich" wrote:

> In my dissertation (Ehrlich 1999), I found that places  
> simulating a "tree of life" with a celestial bird at the  
> top, a crocodile at the base and/or a serpent in the  
> branches often had plants growing nearby. I began to  
> expect to see this species, Cordyline fruticosa, at such  
> places, places where people tried through ceremonies to

> influence powerful supernaturals. This was true of the  
 > Ngaju, the Kenyah, the Iban in Borneo, the Toraja in  
 > Sulawesi, the Karo Batak in Sumatra, the Ifugao in Luzon,  
 > many groups in Papua New Guinea and Island Melanesia and,  
 > of course, in Polynesia.  
 > The journal Anthropos recently rejected an article I  
 > wrote, arguing that the most probable reason for the  
 > phenomenon was that the idea of a reddish plant as a sort  
 > of cinnabar, was that people had continued to believe  
 > they could influence supernaturals by a kind of alchemy.  
 > Isn't this more likely than that the associations between  
 > tree of life, dragon-like supernaturals and ti plants  
 > should have been re-invented repeatedly?  
 > Celia Ehrlich  
 >  
 > -----  
 > Celia Ehrlich  
 > 254 Poverty Lane  
 > Lebanon, NH 03766-2702  
 > cehrich@c...  
 | 148|2002-10-11 16:49:56|Juha Savolainen|A Final Cautionary Brief for Saraswati Seekers|  
 A Cautionary Brief for Saraswati Seekers II  
 (The Last Reply to Paul Kekai Manansala)

PKM: ?What Kivisild et al. showed was that markers  
 alone are  
 useless unless you can show some chronology that  
 matches  
 the assumptions of the study.

The only way to that is to show evidence that the  
 upper  
 castes are derived almost entirely from that  
 "invasion."  
 And not from latter gene flow or caste creation.?

JS: It would have been nice if you had provided the  
 reference?after all, the reference seems to play an  
 important role in your argument. Apropos Kivisild, I  
 cannot help laughing when I notice that indigenists  
 cite eagerly the earlier Kivisild, Bamshad et al.  
 study but curse the later Bamshad, Kivisild et al.  
 study...Hilarious!

Anyway, what you say via reference to Kivisild is



misleading if interpreted generously, false if interpreted more literally. If your ?some chronology? refers to a calibration of a molecular clock via independently dated events, then a qualified assent can be defended. Qualified, because that independent dating need not refer to the specific object of the study, i.e. one does not need to establish the chronology of India to make use of molecular clocks to date events related to Indian history. But that is not all.

Genetics does provide us entirely independent means for dating history: microsatellites are routinely used to assess the time depth of various events. Indeed, it is by means of microsatellites the arrival times of Eu19/HG16/HG3 etc. to India (the different symbols representing different nomenclatures, see [http://hpgl.stanford.edu/publications/GR\\_2002\\_v12\\_p339-348.pdf](http://hpgl.stanford.edu/publications/GR_2002_v12_p339-348.pdf)) have been estimated.

In fact, it is precisely by microsatellites that Ramana, Su et al. (2001) try to show that the observed SNP-haplotype sharing is not due to the great antiquity of the haplotypes in question. Too bad that you ignore this important aspect of the very study you want to present as evidence for the indigenist case. However, if you wake up from your indigenist slumbers, you might want to find out what the microsatellite-based estimates for the arrival of Eu19/HG16/HG3? haplotypes to India actually are. Articles such as

[http://hpgl.stanford.edu/publications/PNAS\\_2001\\_v98\\_p10244.pdf](http://hpgl.stanford.edu/publications/PNAS_2001_v98_p10244.pdf)

<http://www.journals.uchicago.edu/AJHG/journal/issues/v68n2/002418/002418.html>

<http://www.ias.ac.in/jbiosci/nov2001/533.pdf>

will drive the point home: despite large uncertainties in the precise time-frame, the existence of a major gene flow from Eastern Europe (more precisely from the Pontic steppes), probably via the steppes of Central Asia, to India during the Holocene period is a fact. This is just what I have been claiming all along. But if you want to live in denial, be my guest, it is your business...

PKM: ?There is no such thing as an "Indo-European" marker.

Genes are different than language or even "race."

The only thing the Bamshad et al. study shows is that there is some similarity in markers between two regions.

However, these same markers often turn up in higher ratios among non-IE Central Asians than in Europeans.?

JS: Well, this time you have exceeded yourself! I have never confused genes with language or culture ? they certainly are different things.

First, to speak about ?Indo-European markers? is just shorthand here. But I admit that I have underestimated your potential for misinterpreting my words. Indeed, it would be ridiculous to say that carrying Eu19 would in itself testify that one is an Indo-European. It would be ridiculous already because the mutation was probably born before the end of the last Glacial Period and there is no sense in which the first carriers of the mutation would have been Indo-Europeans as the language family did not exist yet. So, when I talk about ?Indo-European markers?, I mean nothing more than that when we study these genes, we find that they are often carried by persons speaking Indo-European languages. We are talking about a correlation that calls for an explanation.

As ancient peoples did not possess any means for digital communication, it follows that linguistic, cultural and genetic migrations etc. overlap to an extent. Sometimes the overlap was considerable, sometimes small. Just how much they overlapped must be always decided in the light of our best evidence. Still, thanks to historical continuities, peoples who have been talking Indo-European languages tend to carry different genes than peoples who speak, say, African or Austronesian languages. It is surely no accident that some segments of Indian population (mainly upper caste and Hindi speaking) are genetically more closely related to Eastern Europeans

than to many Asians. Certain genes are markers for this relatedness and given the likely time frame, it makes sense to assume that the expansion of Indo-European languages contributed to the spread of these markers as well.

Second, it is imperative to remember that we are talking about the non-coding region of our genome: good genetic clocks are selectively neutral. Hence all dark hints about racialism etc. are here entirely misplaced. We are talking about 'junk genes', not about genes coding 'racial' phenotypic properties.

Third, it is certainly true that some non-Indo-European speaking peoples also carry the genes Indo-European speaking peoples tend to carry. Now, if the ancestors of the former migrated to India - to the exclusion of people who spoke Indo-European languages - what languages did they carry with them and when? And how did India become predominantly Indo-European then? By being the home of all Indo-European languages, as the indigenists claim? Let us graciously forget that comparative linguists dismiss the very idea as hopeless. But if indigenists were correct and Indo-European languages were born in India, we should find a gene flow from India to other areas where Indo-European languages have been spoken. But what do we actually find? Where is the greatest diversity of relevant haplotypes and the highest relative representation for their haplogroup? Bluntly, but not unfairly, who is using Occam's razor here properly and who is just mutilating himself with it?

PKM: 'Many Rajput castes may indeed trace their lineage to Hun and other non-IE nomadic peoples who came to India.

That's the problem. There are no "standard methods" used for the "arrival times" in Bamshad et al.

They simply make their case that the arrival was connected with the Aryan invasion based on caste integrity that supposedly existed since that time.

Despite all your excessive use of quotations, you seem to have ignored that main point of my last post.?

JS: No, I am not ignoring anything worth taking seriously. It is you who are out in the woods again. Arrival times to India have been estimated by means of microsatellites and they suggest that the relevant haplogroups appeared in Northern India certainly after the latest Glacial Period and certainly before 100 BCE, most likely during the interval starting from pre-Harappan and ending to post-Harappan times. Given the uncertainties, this is in fair agreement with the mainstream version of Indo-Aryan arrival to India around 1700 to 1200 BCE. Hence my point about the Bamshad, Kivisild et al. study (2001) remains.

PKM: ?Bamshad et al. depended on a single unnamed "Kshatriya" clan to come to its conclusions.

The name of the clan is important to determine the ultimate origin of the group. Is it, for example, a Rajput group that came only in medieval times to South India?

The ancient Kshatriyas of South India were Vratyas or converts known as Dravida. That indicates that most local raja groups in South India are of local native origin.

Again, the Kshatriya caste routinely admitted powerful foreign invaders into their group. So how is it possible to determine whether any particular clan traces all or

any of its heritage to a hypothetical "Aryan invasion?" Especially when the group goes unidentified.

Although there have been many studies on the foreign origin of

many Rajput clans, the case of the Chhetris of Nepal is documented even better.

The indigenous Magar and other lineage in the royal family and other noble Chhetri clans is well-attested in the Nepalese annals. The first members of the Gorkha royal family had Magar names and came from the Magar region.

Nearly all Chhetri clans share clan names with Magar clans, and there are some who even still speak the Magar language.

There are also the so-called Matwali Chhetris who are of recent known mixed ancestry (Gurung, Magar, etc).?

JS: I welcome all detailed case studies. Cheers for you for that. But if you read my references carefully, you will see that the burden of the proof lies mainly on the shoulders of the indigenist school. Of course, many more studies along the lines of Bamshad, Kivisild et al. must be made before we can tell the story with reasonable certainty. However, what we have already learned shows that the indigenists face a truly daunting challenge. Once again, if you want to deny that, be my guest. It is your life and it is up to you whether to open up yourself to true scientific spirit or not.

As for me, well, I do not think that I will continue this exchange on matters related to Mighty Saraswati anymore here: the era of diminishing returns has evidently set in and turned this debate into a waste of time and energy. Indeed, the only fish that seems to be swimming in the 'Mighty Saraswati' is red herring?

And if you want to have the 'last word' here, it is yours for taking. I will continue my commentary on Indian prehistory at the different web site, namely the 'Ma'at':

[www.thehallofmaat.com](http://www.thehallofmaat.com)

However, before my exit from the "Austric" newsgroup, I will finally deal a couple of earlier objections quickly:

PKM: "Besides there are strong biological arguments against the idea that the Indian horse is a relative recent (Copper Age?) introduction from Central Asia."

JS: There are not, this claim is just hot air.

PKM: "You may have missed one important point. How could invading Aryans not know about wheat? Doesn't that go against all standard theories of PIE culture?"

JS: No, not at all! But if the Indo-Aryans did not know wheat and wheat was the staple food in the Harappan culture, where does that leave the suggested identification "Vedic Aryans" equal "Harappans"?

Take care,

Juha Savolainen

---

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| 149|2002-10-11 17:30:45|a.manansala@attbi.com|Re: A Final Cautionary Brief for Saraswati Seekers|

>  
> JS: I welcome all detailed case studies. Cheers for  
> you for that. But if you read my references carefully,  
> you will see that the burden of the proof lies mainly  
> on the shoulders of the indigenist school. Of course,  
> many more studies along the lines of Bamshad, Kivisild  
> et al. must be made before we can tell the story with  
> reasonable certainty. However, what we have already  
> learned shows that the indigenists face a truly  
> daunting challenge. Once again, if you want to deny  
> that, be my guest. It is your life and it is up to you  
> whether to open up yourself to true scientific spirit  
> or not.  
>

Again, all you have done was to avoid the main fault  
(of many) with the Bamshad et al. study.

It relies entirely on caste integrity and not "microsatellite  
dating" to make its point.

You tried to mix results of other irrelevant studies  
using different markers to support Bamshad et al.  
Tsk, tsk.

Regards,

Paul Kekai Manansala

| 150|2002-10-11 21:26:22|TTT UUU|Re: Tree of Life, dragon and Cordyline fruticosa|

Celia

I am interested in your reply regarding Africa as well and, the other side of Africa. I have heard of a Dragon Tree from the Canary Islands. Different species I guess. There was a steppyramid building culture here too. Thor Hayerdahl brought it to world attention before he passed away last year. The structures have become a tourist attraction.

I'd like to get your reprints. I have little contact with botanists here (very isolated in Korean).

Your research plant, Cordyline, is probably too tender for the Korean climate. I have not seen it here. Japan is much milder in the south.

Robin TimDay Dongseo University, English Dept., S. Korea

**Paul Kekai Manansala** wrote:

Celia, I remember you connected this plant earlier with the East African trade mentioned by Schoff.

If the motifs are found out in the Pacific does that suggest that the transfer of the plant took place before the Lapita expansion?

I may have missed it but does the idea of a reddish cinnabar associated with alchemy, dragon-like creatures and the tree of life also occur in the East African context?

Regards,  
Paul Kekai Manansala

--- In austrie@y..., "Celia Ehrlich" wrote:

> In my dissertation (Ehrlich 1999), I found that places  
> simulating a "tree of life" with a celestial bird at the  
> top, a crocodile at the base and/or a serpent in the  
> branches often had ti plants growing nearby. I began to  
> expect to see this species, Cordyline fruticosa, at such  
> places, places where people tried through ceremonies to  
> influence powerful supernaturals. This was true of the  
> Ngaju, the Kenyah, the Iban in Borneo, the Toraja in  
> Sulawesi, the Karo Batak in Sumatra, the Ifugao in Luzon,  
> many groups in Papua New Guinea and Island Melanesia and,  
> of course, in Polynesia.  
> The journal Anthropos recently rejected an article I  
> wrote, arguing that the most probable reason for the  
> phenomenon was that the idea of a reddish plant as a sort  
> of cinnabar, was that people had continued to believe  
> they could influence supernaturals by a kind of alchemy.  
> Isn't this more likely than that the associations between  
> tree of life, dragon-like supernaturals and ti plants  
> should have been re-invented repeatedly?  
> Celia Ehrlich  
>  
> -----  
> Celia Ehrlich  
> 254 Poverty Lane  
> Lebanon, NH 03766-2702  
> cehrich@c...



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| 151|2002-10-11 21:34:25|TTT UUU|Re: Mexican Links to E. Asia|

There is quite a body of evidence buildingsuggesting cultural contact between preColumbianMexico and China or SE Asia. I have not been able to get the book about the Buddhist sea journey to Fusang (Mexico) through Amazon.com. Does anybody have a usedcopy or another source?

**TTT UUU** wrote:

Thanks Paul: Wood construction is/was also predominant in New Guinea as well.

### **Mexican Links**

I am just finishing Oppenheimer's book (on my lap now)and rememberedthe "tree of life" motif and mythic storieswas very prominent among the Maya.It has the same elements: the tree axis (often a cross)with foliage and fruits sometimes, a celestial bird at the top and a crocodileat the base and a serpent in the branches. This motif is often worked in stone or plaster at temple sites. I have several books with theseillustrations and if I remember correctly the most famous would be KingPacals stonetomb lid at Palenque. This to me is no coincidence. Austronesians must have crossed the Pacific and introduced some of their technology.There was no bronze work among the early Maya/Mexicans but it appeared later among the Mexitic in a limited areaso presumably this technology was not transfered at an early period. [The Inca also had bronze for ceremonialuse among the court.]The Mexicans also made paper bark cloth alsofrom Mulberry, and I have commented earlier that

sweet potato was probably carried back across the Pacific from S. American canoe by the Polynesians, or earlier Austonesians.

Oppenheimer (photo 26 & 27) points out bronze ( + pottery base) money trees in Sichuan Chinese burials and these had birds in the branches and a monster at the base. Surprise! Similar figures/sculptures were excavated in the Paekjae kingdom of west Korea. A very famous bronze lidded incense burner is crafted in this fashion also with a dragon at the base. Should be easy to find this illustration with a web search. The bronze mask photo 25 also found in Sichuan is almost identical to an object (flag pole cap) from the same Korean Paekjae culture. In China this mask is known as the T'ao-t'iet or Glutton. It is associated with human sacrifice. The lower jaw is often not illustrated. The story goes that the glutton often associated with human death from drought lost its lower jaw as a punishment. The Glutton mask is commonly illustrated on old Chinese and Vietnamese bronze dynastic cauldrons. These cauldrons held wine or blood at different times. As time passed the mask became more abstract and is harder to recognize. Later versions have a long undulating nose and look so much like the Yucatan Chac god masks seen all over some Mayan temples (eg. Labna, Chichen Itza, etc.). Another surprise: these Mayan examples have no jaw and also bear the undulating nose. In Mexico the Chac god was appealed to for rain also associated with drought and human death. In northern areas of Mexico Chac is known by the name of Tlaloc. I guess we have the working hypothesis that the Chinese T'ao-t'ieh is in fact the Mexican Tlaloc/Chac and was introduced to Mexico's Pacific coast by Austronesians, master mariners.

Will cite some of these illustrated books I have later.

Regards

Robin Day S. Korea

*a.manansala@attbi.com* wrote:

- >
- > Robin, Paul,
- >
- > Thanks for all the information. By the way, as for the other areas in the
- > archipelago, it is a theory that the majority of pre-contact religious sites were
- > made of wood and not stone, as compared to other pre European contact
- > civilizations in South America and Southeast Asia.

>

The early-contact European writings confirm that most of the large buildings in the Philippines and Borneo were constructed of wood.

Regards,  
Paul Kekai Manansala

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| 152|2002-10-11 21:51:38|TTT UUU|Re: TOL, dragon and Cordyline fruticosa|

Paul

Oppenheimer gives lots of evidence that there was an eastern expansion of Austronesian people(s) long before the Lapita Pottery Cultural expansion into the east Pacific. The last great sea flood occurred suddenly at the end of the last ice age. The widespread dragon motif is believed to be a direct result of the expansion of the lethal salt-water crocodile into flooded villages. These crocs have been found as far east as Fiji.

Robin Tim Day S. Korea

The Tree of Life myth is very old in SE Asia.

**Paul Kekai Manansala** wrote:

Celia, I remember you connected this plant earlier with the East African trade mentioned by Schoff.

If the motifs are found out in the Pacific does that suggest that the transfer of the plant took place before the Lapita expansion?

I may have missed it but does the idea of a reddish cinnabar associated with alchemy, dragon-like creatures and the tree of life also occur in the East African context?

Regards,  
Paul Kekai Manansala

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> simulating a "tree of life" with a celestial bird at the  
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> expect to see this species, *Cordyline fruticosa*, at such  
> places, places where people tried through ceremonies to  
> influence powerful supernaturals. This was true of the  
> Ngaju, the Kenyah, the Iban in Borneo, the Toraja in  
> Sulawesi, the Karo Batak in Sumatra, the Ifugao in Luzon,  
> many groups in Papua New Guinea and Island Melanesia and,  
> of course, in Polynesia.  
> The journal *Anthropos* recently rejected an article I  
> wrote, arguing that the most probable reason for the  
> phenomenon was that the idea of a reddish plant as a sort  
> of cinnabar, was that people had continued to believe  
> they could influence supernaturals by a kind of alchemy.  
> Isn't this more likely than that the associations between  
> tree of life, dragon-like supernaturals and ti plants  
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> Celia Ehrlich

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> -----  
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> cehrlich@c...

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| 153|2002-10-12 08:00:49|a.manansala@attbi.com|Re: TOL, dragon and Cordyline fruticosa|

>  
> Paul  
> Oppenheimer gives lots of evidence that there was an eastern expansion of  
> Austronesian people(s) long before the Lapita Pottery Cultural expansion into  
> the east Pacific. The last great sea flood occurred suddenly at the end of the  
> last ice age. The widespread dragon motif is believed to be a direct result of  
> the expansion of the lethal salt-water crocodile into flooded villages. These  
> crocs have been found as far east as Fiji.  
> Robin Tim Day S. Korea  
>

The date of the Lapita expansion is of interest to me here  
because that's about when I believe there are some preliminary  
indications that Austronesian contact was made with East  
Africa.

Regards,  
Paul Kekai Manansala  
| 154|2002-10-13 09:39:59|Jojo Malig|Re: Digest Number 63|

I have read of the East African connection. If the Austric expansion indeed predates Lapita cultural expansion, then we can predate the Austronesian expansion to earlier calculated dates.

Jojo Malig

*austric@yahoogroups.com* wrote:

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There is 1 message in this issue.

Topics in this digest:

1. Re: Re: TOL, dragon and Cordyline fruticosa  
From: [a.manansala@attbi.com](mailto:a.manansala@attbi.com)

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Message: 1  
Date: Sat, 12 Oct 2002 15:00:46 +0000  
From: [a.manansala@attbi.com](mailto:a.manansala@attbi.com)  
Subject: Re: Re: TOL, dragon and Cordyline fruticosa

>  
> Paul  
> Oppenheimer gives lots of evidence that there was an eastern expansion  
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> Austronesian people(s) long before the Lapita Pottery Cultural

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Africa.

Regards,  
Paul Kekai Manansala

---

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"Most people are other people - their lives a mimicry, their passions, a quotation."  
- Oscar Wilde

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<http://twentytwenty.blogspot.com/>  
<http://pampangaonline.com>

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| 155|2002-10-13 17:39:10|TTT UUU|Re: Madagascar, E. Africa, Australia Americas|

Jojo

There were many expansions of the Austric or Austronesian people ancient and modern. They are still on the move in the present. Oppenheimer says genetic markers show that south Asians (Amerind speakers) colonized most of North and South America. This had to be a long way back into the last ice age. He suggests they did it by sea migration along the North Pacific coast. Later groups occupy the north of North America, principally, (Inuit and Dene-Navajo).

The colonization of Madagascar and introduction of rice culture by Austric people was certainly epic. I ask my Korean students "What is the only Asian speaking nation in Africa?" and they never know the answer. My fellow Canadians are equally ignorant of this amazing history.

As far as we know the Madagascar people never started an agriculture community in east Africa and the Austric people never started similar agricultural communities in north Australia although their genes ARE in Australia. It's possible there was a clash of culture with the existing peoples.

*a.manansala@attbi.com* wrote:

>  
> Paul  
> Oppenheimer gives lots of evidence that there was an eastern expansion  
of  
> Austronesian people(s) long before the Lapita Pottery Cultural  
expansion into  
> the east Pacific. The last great sea flood occurred suddenly at the end of  
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> last ice age. The widespread dragon motif is believed to be a direct  
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Regards,  
Paul Kekai Manansala

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| 156|2002-10-13 17:56:30|TTT UUU|Re: T of L, dragon/croc and Cordyline fruticosa|

Celia

mentioned in private e-mail that a sea monster Tiamat was alsoan important mother god in Sumeria and that the Nile valley alsohas a lethalcroc (not a salt water species). Oppenheimer discusses both of these subjects, especially Tiamat which may stem from Austronesian myths of saltwater crocs.

The Polynesian journals she mentionsare not available in Korean universities (poorly stocked with internationaljournals in general).Photocopies? We can trade.

*a.manansala@attbi.com* wrote:

>  
> Paul  
> Oppenheimer gives lots of evidence that there was an eastern expansion  
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> Austronesian people(s) long before the Lapita Pottery Cultural  
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Regards,  
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| 157|2002-10-13 23:28:00|TTT UUU|Re: Bombing in Bali|

One of the teachers at our University is missing in Bali and two sisters ( Korean wife and sister-in-law) of another teacher are both missing. Dental records are being sent to Bali. Sometimes the news seems far away but it has struck very close this time.

I feel so unhappy for what happened and unhappy for Indonesia. Who will visit your country now?

Robin Tim Day S. Korea

*a.manansala@attbi.com* wrote:

>  
> Paul  
> Oppenheimer gives lots of evidence that there was an eastern expansion  
of  
> Austronesian people(s) long before the Lapita Pottery Cultural  
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> the east Pacific. The last great sea flood occurred suddenly at the end of  
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Here's something on the Tiamat ( saltwater crocodile) in Mesopotamian mythology.

One question: Does the Eastern (Chinese) dragon represent crocodiles likewise? And remember the Rokuru? The Japanese dragon jars with similarities with the eiones made in Luzon? Alas, in the Philippines, today the crocodile is allegorically something else: a government official.

cheers,  
Jojo

<http://www.pantheon.org/articles/t/tiamat.html>

In Babylonian myths, Tiamat is a huge, bloated female dragon that personifies the saltwater ocean, the water of Chaos. She is also the primordial mother of all that exists, including the gods themselves.

Her consort is Apsu, the personification of the freshwater abyss that lies beneath the Earth. From their union, saltwater with freshwater, the first pair of gods were born. They are Lachmu and Lachamu, parents of Anshar and Kishar, grandparents of Anu and Ea.

In the creation epic Enuma elish, written around 2000 BCE, their descendants started to irritate Tiamat and Apsu so they decided to kill their offspring. Ea discovered their plans and he managed to kill Apsu while the latter was asleep. Tiamat flew into a rage when she learned about Apsu's death and wanted to avenge her husband. She created an army of monstrous creatures, which was to be led by her new consort Kingu, who is also her son. Eventually, Tiamat was defeated by the young god Marduk, who was born in the deep freshwater sea.

Marduk cleaved her body in half, and from the upper half he created the sky and from the lower half he made the earth. From her water came forth the clouds and her tears became the source of the Tigris and the Euphrates. Kingu also perished, and from his blood Marduk created the first humans.

| 159|2002-10-14 22:59:47|a.manansala@attbi.com|Re: Tiamat|

- > Her consort is Apsu, the personification of the freshwater abyss that
- > lies beneath the Earth. From their union, saltwater with freshwater,
- > the first pair of gods were born. They are Lachmu and Lachamu,

These gods Lachmu and Lachamu are associated in Neo-Sumerian art with water buffalo.

Of course, the water buffalo is not an animal of the ancient Fertile Crescent. Like the tiger and rhinoceros of Neo-Sumerian art it shows at least a South Asian influence.

Regards,

Paul Kekai Manansala

| 160|2002-10-15 19:43:35|TTT UUU|Re: Tiamat & rino and water buffalo |

Paul

What do you mean by Neo-Sumerian art and where have you seen these illustrations with waterbuffalo, tigers and rino? ceramics I am guessing. All very interesting.

One thing that bothers me about so many publishers is that they do not state the location of many of the archaeological objects. Readers would like to know the museum or if its in a private collection.

*a.manansala@attbi.com* wrote:

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| 161|2002-10-15 19:51:47|TTT UUU|Re: Tiamat/Crocs|

China presently has a very small and shy alligator in the south. It is quite rare. I saw live specimens in the Hong Kong Zoo, near Victoria Park. This animal may have been more widespread in the north in warmer periods. The dragon in China is generally seen as benevolent and a symbol of the power of the marshes/wetlands. This friendly attitude of the Chinese may derive from this small and mostly harmless animal. Chinese prob. had little contact with sea crocodiles except on trade voyages.

I thought that most dragon jars came from China trading ships. Japan too?

*jojomalig* wrote:

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| 162|2002-10-15 19:58:11|TTT UUU|Re: Madagascar, E. Africa, Australia Americas|

I think this is quite a mystery: Why was simple tropical root and tuber agriculture never introduced into north Australia?

I read in a National Geographic I just picked up (reference tomorrow) that the local people were using grinding stones to make a flour of wild seeds. These are the oldest grinding stones known, 30,000 PB. Of course the grinding stone may have been used to prepare paint, not food!

**TTT UUU** wrote:

Jojo

There were many expansions of the Austric or Austronesian people ancient and modern. They are still on the move in the present.

Oppenheimer says genetic markers show that south Asians (Amerind speakers) colonized most of North and South America. This had to be a long way back into the last ice age. He suggests they did it by sea migration along the North Pacific coast. Later groups occupy the north of North America, principally, (Inuit and Dene-Navajo).

The colonization of Madagascar and introduction of rice culture by Austric people was certainly epic. I ask my Korean students "What is the only Asian speaking nation in Africa?" and they never know the answer. My fellow Canadians are equally ignorant of this amazing history.

As far as we know the Madagascar people never started an agriculture community in east Africa and the Austric people never started similar agricultural communities in north Australia although their genes ARE in Australia. It's possible there was a clash of culture with the existing peoples.

*a.manansala@attbi.com* wrote:

- >
- > Paul
- > Oppenheimer gives lots of evidence that there was an eastern expansion of
- > Austronesian people(s) long before the Lapita Pottery Cultural expansion into
- > the east Pacific. The last great sea flood occurred suddenly at the end of the
- > last ice age. The widespread dragon motif is believed to be a direct result of
- > the expansion of the lethal salt-water crocodile into flooded villages. These
- > crocs have been found as far east as Fiji.
- > Robin Tim Day S. Korea
- >

The date of the Lapita expansion is of interest to me here because that's about when I believe there are some preliminary indications that Austronesian contact was made with East Africa.

Regards,  
Paul Kekai Manansala



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| 163|2002-10-16 17:26:30|TTT UUU|Re: Madagascar, E. Africa, Australia Americas|

See p.102-103 of the National Geographic, Vol. 189 (1), July 2000 for a picture of the Australian grinding stone.

**TTT UUU** wrote:

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| 164|2002-10-16 18:01:15|Jojo Malig|Re: Japanese dragon jars|

---

Japanese dragon jars were called Rusun tsukuru and they are quite rare indeed. The interesting thing is the pre-Spanish contact trade between Japan and Luzon villages, as dragon jars were also found in possession of prominent families the island. Rusun=Luzon.

I thing Paul has written something on this already.

regards,

Jojo

---

---

---

Message: 2

Date: Tue, 15 Oct 2002 22:51:46 -0400 (EDT)

From: TTT UUU

Subject: Re: Tiamat/Crocs

China presently has a very small and shy aligator in the south. It is quite rare. I saw live specimens in the Hong Kong Zoo, near Victoria Park. This animal may have been more widespread in the north in warmer periods. The dragon in China is generally seen as benevolent and a symbol of the power of the marshes/wetlands. This friendly attitude of the Chinese may derive from this small and mostly harmless animal. Chinese prob. had little contact with sea crocodiles except on trade voyages. I thought that most dragon jars came from China trading ships. Japan too? jojomalig wrote:Here's something on the Tiamat ( saltwater crocodile) in Mesopotamian mythology.

One question: Does the Eastern (Chinese) dragon represent crocodiles likewise? And remember the Rokuru? The Japanese dragon jars with similarities with the eiones made in Luzon? Alas, in the Philippines, today the crocodile is allegorically something else: a government official.

cheers,  
Jojo

<http://www.pantheon.org/articles/t/tiamat.html>

In Babylonian myths, Tiamat is a huge, bloated female dragon that personifies the saltwater ocean, the water of Chaos. She is also the primordial mother of all that exists, including the gods themselves.

Her consort is Apsu, the personification of the freshwater abyss that lies beneath the Earth. From their union, saltwater with freshwater, the first pair of gods were born. They are Lachmu and Lachamu, parents of Ansar and Kisar, grandparents of Anu and Ea.

In the creation epic Enuma elish, written around 2000 BCE, their descendants started to irritate Tiamat and Apsu so they decided to kill their offspring. Ea discovered their plans and he managed to kill Apsu while the latter was asleep. Tiamat flew into a rage when she learned about Apsu's death and wanted to avenge her husband. She created an army of monstrous creatures, which was to be led by her new consort Kingu, who is also her son. Eventually, Tiamat was defeated by the young god Marduk, who was born in the deep freshwater sea.

Marduk cleaved her body in half, and from the upper half he created the sky and from the lower half he made the earth. From her water came forth the clouds and her tears became the source of the Tigris and the Euphratus. Kingu also perished, and from his blood Marduk created the first humans.

"Most people are other people - their lives a mimicry, their passions, a quotation."  
- Oscar Wilde

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| 165|2002-10-16 18:09:41|TTT UUU|Re: Japanese dragon jars|

Jojo

What is your main interest, archaeology, linguistics? Can you suggest a good book covering Austric archaeology? I am looking today for one about the Ainu of Japan.

The National Geographic issue I mentioned also shows the massive sea flood of the Black Sea at about 7500 BP and also the map of the world at the time of the ice age. SE Asia had not been flooded and Australia was joined to N. Guinea etc. Very nice map.

Robin Day S. Korea

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Rusun=Luzon.

I thing Paul has written something on this already.

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Jojo

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"Most people are other people - their lives a mimicry, their passions, a quotation." - Oscar Wilde

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| 166|2002-10-16 19:34:46|a.manansala@attbi.com|Re: Tiamat & rino and water buffalo|  
Neo-Sumerian refers to the period 2100-2000 BCE.

Here is a link showing the water buffalo with Lachmu.

You can find some of the other images along with many  
from Harappa at:

<http://www.hindunet.org/saraswati/smithy1.htm>

Regards,  
Paul Kekai Manansala

>

> Paul

> What do you mean by Neo-Sumerian art and where have you seen these

> illustrations with waterbuffalo, tigers and rino? ceramics I am guessing. All

> very interesting.

> One thing that bothers me about so many publishers is that they do not state the

> location of many of the archaeological objects. Readers would like to know the

> museum or if its in a private collection.

>

> [a.manansala@attbi.com](mailto:a.manansala@attbi.com) wrote:

>

>> Her consort is Apsu, the personification of the freshwater abyss that

>> lies beneath the Earth. From their union, saltwater with freshwater,

>> the first pair of gods were born. They are Lachmu and Lachamu,

>

> These gods Lachmu and Lachamu are associated in Neo-Sumerian  
> art with water buffalo.

>

> Of course, the water buffalo is not an animal of the ancient

> Fertile Crescent. Like the tiger and rhinoceros of Neo-Sumerian

> art it shows at least a South Asian influence.

>

> Regards,

> Paul Kekai Manansala

>

>

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| 167|2002-10-16 19:43:26|a.manansala@attbi.com|Re: Japanese dragon jars|

>

> \_\_\_\_\_

>

> Japanese dragon jars were called Rusun tsukuru and they are quite rare indeed.

> The interesting thing is the pre-Spanish contact trade between Japan and Luzon

> villages, as dragon jars were also found in possession of prominent families the

> island. Rusun=Luzon.

>

> I thing Paul has written something on this already.

>

>

Jojo most of the Rusun-tsukuru exported to Japan, afaik,  
were the ordinary crude water pots of the Philippine home.

These were considered to have extraordinary ability to keep  
water cool and bubbly by their ability to "sweat."

The sweating quality also allowed the growth of lichens  
inside the jar making the water contained therein "sweet."

The dragon jars were probably manufactured in the Philippines  
although of obvious Chinese influence. The true Philippine  
type is extremely rare or absent in China. I believe a  
shipful of these was recovered though off the coast of  
Korea. A sign of medieval trade?

Regards,  
Paul Kekai Manansala

Regards,

P

| 168|2002-10-16 19:46:23|a.manansala@attbi.com|Fwd: Ancient Man Bac people discovered in  
Vietnam|

Voice of Vietnam  
Ancient Man Bac people discovered

Archaeologists from the Vietnam Archaeology Institute (VNAI) have found 10 ancient tombs containing the remains of 11 Man Bac people, half of them children, at the second excavation in Hien Thanh commune, Yen Mo district of northern Ninh Binh province. They also unearthed around 50 axes, 75 graters, eight chisels, six necklaces, three pottery pots and copper bowls, ten rings and hundreds of kg of mollusc shells. The first excavation was conducted in 1999.

Archaeologists said the relics found in the two excavations relate to periods of the late Phung Nguyen and early Dong Dau cultures, dating back to around 4,000 years. Three of the ancient Man Bac people were believed to suffer from buzzing - a common disease of ancient people living in the Asia-Pacific region as concluded by Japanese Prof. Kazumichi Katayama in 1998.

Deputy Director of the Institute, Prof. Ha Van Phung say the antiquities proved the ancient Man Bac people had lived in mountainous region connecting with the sea. In the early age, their livelihood depended on gathering, hunting, cultivation and seafood.

Local authorities plan to build an outdoor museum on this excavation site.

| 169|2002-10-17 16:42:37|Jojo Malig|Re: Austric linguistics|

Message: 3

Date: Wed, 16 Oct 2002 21:09:41 -0400 (EDT)

From: TTT UUU <[gymnogoy@yahoo.ca](mailto:gymnogoy@yahoo.ca)>

Subject: Re: Japanese dragon jars

Robin,

My main interest is ethnolinguistics. And that is not merely confined on languages but on cultures and traditions of pre-European contact Austronesians.

A good book on Austric archeology? There are numerous resources found online although I recommend the works of ANU Prof. Peter Bellwood.

National Geographic is doing great work still on archeology and anthropological studies.

regards,

Jojo Malig

Philippines

Jojo

What is your main interest, archaeology, linguistics? Can you suggest a good book covering Austric archaeology? I am looking today for one about the Ainu of Japan.

The National Geographic issue I mentioned also shows the massive sea flood of the Black Sea at about 7500 BP and also the map of the world at the time of the ice age. SE Asia had not been flooded and Australia was joined to N. Guinea etc. Very nice map.

Robin Day S. Korea

"Most people are other people - their lives a mimicry, their passions, a quotation." - Oscar Wilde

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| 170|2002-10-17 17:17:17|TTT UUU|Re: Fwd: Ancient Man Bac people discovered in Vietnam|

Tell us more about the disease "buzzing" if you know.

***a.manansala@attbi.com*** wrote:

Voice of Vietnam

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| 171|2002-10-19 04:36:59|TTT UUU|Re: Austric linguistics|

I have not read Bellwood's arguments yet (but saw his book advertized on Amazon.com.) Oppenheimer counters them again and again pointing out linguistic, genetic, mythic and cultural evidence for a centrifugal spread of Austric people from the SE Asia. he suggests many of the

Austric languages today stranded in the upper reaches of Thai, Myanmar, Vietnam and Chinese rivers are there because Austric peoples migrated up these river valleys at the last time of sea flooding 7,500 BP.

Date: Wed, 16 Oct 2002 21:09:41 -0400 (EDT)  
From: TTT UUU <[gymnogoy@yahoo.ca](mailto:gymnogoy@yahoo.ca)>  
Subject: Re: Japanese dragon jars

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Jojo Malig

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| 172|2002-10-20 04:23:18|Jojo Malig|Re: Digest Number 69|

Bellwood presents his hypotheses based on solid facts and material evidence. That I know not of Oppenheimer. Do read Bellwood.

cheers,

jojo

***austrie@yahoo.com*** wrote:

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There is 1 message in this issue.

Topics in this digest:

1. Re: Austric linguistics  
From: TTT UUU

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Message: 1  
Date: Sat, 19 Oct 2002 07:36:54 -0400 (EDT)  
From: TTT UUU  
Subject: Re: Austric linguistics

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[This message contained attachments]

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| 173|2002-10-20 07:57:56|a.manansala@attbi.com|Re: Digest Number 69|  
Personally, I prefer Wilhelm Solheim to Bellwood. I  
think the latter ignores too much evidence.

Oppenheimer is best in the area of his expertise --  
genetics and the biological sciences. However, he has  
done a great job with comparative mythology also.

Regards,  
Paul Kekai Manansala

>

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> I know not of Oppenheimer. Do read Bellwood.

> cheers,

> jojo

>

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>  
> There is 1 message in this issue.  
>  
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>  
> 1. Re: Austric linguistics  
> From: TTT UUU  
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> languages but on cultures and traditions of pre-European contact Austronesians.  
>  
> A good book on Austric archeology? There are numerous resources found online  
> although I recommend teh works of ANU Prof. Peter Bellwood.  
>  
> National Geographic is doing great work still on archeology and anthropological  
> studies.  
>  
> regards,  
>

> Jojo Malig  
>  
> Philippines  
>  
> Jojo  
> What is your main interest, archaeology, linguistics? Can you suggest  
> a good book covering Austric archaeology? I am looking today for one  
> about the Ainu of Japan.  
> The National Geographic issue I mentioned also shows the massive sea  
> flood of the Black Sea at about 7500 BP and also the map of the world at  
> the time of the ice age. SE Asia had not been flooded and Australia was  
> joined to N. Guinea etc. Very nice map.  
> Robin Day S. Korea  
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>  
> "Most people are other people - their lives a mimicry, their passions, a  
> quotation." - Oscar Wilde  
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| 174|2002-10-22 19:18:55|TTT UUU|Re: Solheim, Jomon, Ainu|

Does Solheim have a good book I can get? Saw just the one thing on Amazon, a journal reprint.

Reading more in the July 200 national Geographic and it says the Ainu are direct decendants of the Jomon culture. Is this idea contested?

I am just starting to read more of the Jomon and Japan'sAinu. The Jomonculture was in Cheju Island, S. Korea and Japan. Elsewhere? Oppenheimer p.142says Jomon pottery has been found very far south, 5000 years back, in in the Melanesian areaVanuatu. This suggests far ranging sea connections.

Robin Day

*a.manansala@attbi.com* wrote:

Personally, I prefer Wilhelm Solheim to Bellwood. I think the latter ignores too much evidence.

Oppenheimer is best in the area of his expertise -- genetics and the biological sciences. However, he has done a great job with comparative mythology also.

Regards,  
Paul Kekai Manansala

>

> Bellwood presents his hypotheses based on solid facts and material evidence. That

> I know not of Oppenheimer. Do read Bellwood.

> cheers,

> jojo

>

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>

> There is 1 message in this issue.

>

> Topics in this digest:

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> 1. Re: Austric linguistics

> From: TTT UUU

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> Message: 1

> Date: Sat, 19 Oct 2002 07:36:54 -0400 (EDT)

> From: TTT UUU

> Subject: Re: Austric linguistics

>

>

> I have not read Bellwood's arguments yet (but saw his book advertized on

> Amazon.com.) Oppenheimer counters them again and again pointing out linguistic,

> genetic, mythic and cultural evidence for a centrifugal spread of Austric people

> from the SE Asia. he suggests many of the Austric languages today stranded in > the upper reaches of Thai, Myanmar, Vietnam and Chinese

rivers are there because

> Austric peoples migrated up these river valleys at the last time of sea flooding

> 7,500 BP.

>

>

> Date: Wed, 16 Oct 2002 21:09:41 -0400 (EDT)

> From: TTT UUU

> Subject: Re: Japanese dragon jars

>

>

> Robin,

>

> My main interest is ethnolinguistics. And that is not merely confined on

> languages but on cultures and traditions of pre-European contact Austronesians.

>

> A good book on Austric archeology? There are numerous resources found online

> although I recommend the works of ANU Prof. Peter Bellwood.

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> studies.

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> regards,

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| 175|2002-10-28 08:41:45|Paul Kekai Manansala|Genetics article with some information on  
"Aryan invasion"|  
The following abstract is interesting with regard to recent  
discussion of the "Aryan invasion."

Regards,  
Paul Kekai Manansala

---

Tissue Antigens 2002 Aug;60(2):111-21

Population genetic relationships between Mediterranean populations  
determined by HLA allele distribution and a historic perspective.

Arnaiz-Villena A, Gomez-Casado E, Martinez-Laso J.

HLA genes allele distribution has been studied in Mediterranean and

sub-Saharan populations. Their relatedness has been tested by genetic distances, neighbour-joining dendrograms and correspondence analyses. The population genetic relationships have been compared with the history of the classical populations living in the area. A revision of the historic postulates would have to be undertaken, particularly in the cases when genetics and history are overtly discordant. HLA genomics shows that: 1) Greeks share an important part of their genetic pool with sub-Saharan Africans (Ethiopians and west Africans) also supported by Chr 7 Markers. The gene flow from Black Africa to Greece may have occurred in Pharaonic times or when Saharan people emigrated after the present hyperarid conditions were established (5000 years B.C.). 2) Turks (Anatolians) do not significantly differ from other Mediterraneans, indicating that while the Asians Turks carried out an invasion with cultural significance (language), it is not genetically detectable. 3) Kurds and Armenians are genetically very close to Turks and other Middle East populations. 4) There is no HLA genetic trace of the so called Aryan invasion, which has only been defined on doubtful linguistic bases. 5) Iberians, including Basques, are related to north-African Berbers. 6) Present-day Algerian and Moroccan urban and country people show an indistinguishable Berber HLA profile.

| 176|2002-10-28 08:42:47|Paul Kekai Manansala|Re: Solheim, Jomon, Ainu|

--- In austriac@y..., TTT UUU wrote:

>

> Does Solheim have a good book I can get? Saw just the one thing on

Amazon, a journal reprint.

Nothing comes to mind. Most of his best written work is in journals especially \_Asian Perspectives\_.

Regards,

Paul Kekai Manansala

| 177|2002-10-28 08:45:26|Paul Kekai Manansala|Re: Fwd: Ancient Man Bac people discovered in Vietnam|

--- In austriac@y..., TTT UUU wrote:

>

> Tell us more about the disease "buzzing" if you know.

An ear disease also known as tinnitus.

Regards,

Paul Kekai Manansala

| 178|2002-10-28 18:55:15|TTT UUU|Re: Fwd: Ancient Man Bac people discovered in Vietnam|

Ok tinnitus I know. You said Prof. Katayama concluded this was a common disease in the area. Any idea how this would be caused.....parasites and how would it show from skeletal material? I have not seen the 1998 article. *Paul Kekai Manansala* wrote:

--- In austrie@y..., TTT UUU wrote:

>

> Tell us more about the disease "buzzing" if you know.

An ear disease also known as tinnitus.

Regards,  
Paul Kekai Manansala

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| 179|2002-10-28 19:07:26|TTT UUU|Re: Solheim, Nagas in India, Ainu|

I just ordered **Asian Perspectives Fall 1998 No. 2** from Amazon. It contains W. Solheim material.

**The Nagas: Hill people of N.E. India.** I also ordered this as these people may have migrated from the Austrie regions in the distant past.

*Paul Kekai Manansala* wrote:

--- In austrie@y..., TTT UUU wrote:

>

> Does Solheim have a good book I can get? Saw just the one thing on Amazon, a journal reprint.

Nothing comes to mind. Most of his best written work is in journals especially \_Asian Perspectives\_.

Regards,  
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| 180|2002-10-28 19:18:06|TTT UUU|Re: Ainu books, Japanese links |

Found and ordered the following three on Amazon.com

**Ainu: Spirit of a Northern People** (Anthropological book)

**Harukor: An Ainu Woman's Tale** (fiction based on anthropology)

**Our Land was a Forest: An Ainu Memoir**

**Part of the present day Japanese are thought to have migrated from the south (along the Okinawa chain of islands?) Part is genetically mixed with Ainu and there was considerable addition of Koreans at several times (invasion of Kyushu in the south from the region of Busan), Paekja refugees from the Puyo-Gwangju area and the master potters on the south coast seized and transported by Hideyoshi's troops.**

*Paul Kekai Manansala* wrote:

--- In [austrie@y...](mailto:austrie@y...), TTT UUU wrote:  
>  
> Does Solheim have a good book I can get? Saw just the one thing on

Amazon, a journal reprint.

Nothing comes to mind. Most of his best written work is in journals especially \_Asian Perspectives\_.

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| 181|2002-10-31 18:33:44|TTT UUU|India Cave painting 40,000 BP?|

Reported in The Korean Times p. 15 Oct. 31st.with a photo

"A 40,000-year-old cave painting on a white silica sandstone rock shelter depicting the existence of human civilization is seen in the Banda district 800 kilometers southeast of New Delhi, India, Sunday. The painting shows hunting by cave men in the Paleolithic age."

AP-Yonhap

Strange thing is the painting shows what can only be a horse with a man on or beside it.

I'll write this newspaper as horses are not native to India and domestication was much later.

---

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| 182|2002-11-06 15:16:05|a.manansala@attbi.com|Fwd: Thais Find Ancient Cemetery in Hunt for Origins|

Thais Find Ancient Cemetery in Hunt for Origins

[http://abcnews.go.com/wire/SciTech/reuters20021029\\_8.htm](http://abcnews.go.com/wire/SciTech/reuters20021029_8.htm)

1

Oct. 29

? LOPBURI, Thailand (Reuters) - Archaeologists have discovered an ancient cemetery in central Thailand, one of the largest ever found in the country and a potential key to help unlock the origin of the Thai people.

Artifacts and human remains have been unearthed at the site, believed to date back more than 2,000 years, including items made of bronze, iron, sea shell, marine turtle shell, glass and stone.

Archaeologist Suraphol Nathapintha, a lecturer at Bangkok's Silpakorn University, told Reuters the cemetery could contain up to 10,000 skeletons.

The bones were about 2,200 years old, according to carbon dating, he said.

"From our excavation up to this moment, we have found that this archaeology site seems to be the largest cemetery so far that has been found in central Thailand," Suraphol said.

"From the rough calculation, the cemetery probably contains more than 10,000 skeletons. This large cemetery is certainly related to a very large settlement, so I consider this is very important archaeological evidence," Suraphol said.

The site is 150 km (93 miles) north of Bangkok and he said a settlement with at least 10,000 people during that time in Thailand was considered very large.

The team, with cooperation from the Italian Institute for the Study of Africa and the Far East, plans to start the excavation as a joint project in December. Suraphol estimates it will take about five years to complete the study.

"I personally believe at this moment that this kind of settlement developed in the early history of Thailand, so it can help us clarify the problem where the Thai people come from," Suraphol said.

The team was now studying the bones, including measuring them to calculate the height of the people in the settlement, he said.

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| 183|2002-11-07 17:15:37|jojomalig|Underwater Megaliths off Japan|  
Stone Monuments or Natural Geology?  
Japan's Mysterious Submerged Stone Structures:

New evidence suggests they may have been used by Man  
by Laura Lee

Fotos: <http://www.lauralee.com/japan/japan1.htm>  
<http://www.lauralee.com/japan/japan2.htm>

Man-made, made by Nature, or did humankind finish what Nature started? These enigmatic, sunken stone structures off Okinawa, Japan, located 60 to 100 feet beneath the ocean surface, have the Japanese wondering if their homeland was once part of the lost continent of Mu.

Stone terraces, right angled block and walls, and stone circles encompassing hexagonal columns look intriguingly, if not conclusively, man made. A few more clues: an encircling road, what might be post holes supported long-gone wooden structures, what look like cut steps, and castles with similar architecture located nearby and still on land. (see photos; link at end of this article)

The two sites that are getting the most attention: near the city of Naha is Okinawa is what looks like a wall, with a coral encrusted right angled block. Another, just off the southern end of the tiny island of Yonaguni, the southernmost island of Japan, is an extensive site, with five irregular layers that look like ceremonial, terraced platforms. There are eight anomalous, underwater sites found to date.

Prof. Masaaki Kimura, a marine geologist with the University of the Ryukyus in Okinawa has spent several years studying all eight sites, especially Yonaguni, which was found 13 years ago, in 1985.

Kimura believes these are monuments made by man, left by an unknown

civilization, perhaps from the Asian mainland, home of our oldest civilizations. He reasons that if the five layers on the Yonaguni site had been carved by nature, you would find debris from the erosion to have collected around the site, but no rock fragments have yet been found. He adds that there is what look like a road encircling the site as further indication it was used by man. He believes building this monument necessitated a high degree of technology, and some sort of machinery.

How to date these sites? A few possible scenarios have been suggested. The sites may have been submerged when sea levels rose at the end of the last Ice Age as the continental ice sheets melted. Or, as Japan sits on the Ring of Fire, tectonic activity might have caused subsidence of the land. Or perhaps a combination of subsidence and inundation from rising sea levels, or some catastrophic event, dropped it, intact and upright, into the ocean. Teruaki Ishii, a professor of geology at Tokyo University, believes the site is partly man-made, partly natural, and suggests a date of 8,000 B.C., contemporary to the ancient civilizations of Mesopotamia and the Indus Valley. Others have suggested a date of 12,000 years.

The preliminary reports from the first Americans to dive the sites:

Just back (May 1998) from diving two of the eight known sites are Mike Arbuthnot, an amateur underwater archeologist adventurer, and Boris Said, Executive Producer of the NBC documentary, "Mystery of the Sphinx." Both are experienced divers. Arbuthnot explored a three-mast schooner wrecked off Grand Cayman Island, and Said has been diving for 40 years.

It was treacherous terrain even for experienced divers. "The Yonaguni site is fairly near the shore, so there was heavy surge (the up and down motion of waves) as well as swift currents, and sharks," says Arbuthnot. "On the up side, the area has the third clearest water in the world, with visibility to 200 feet. And the corals were gorgeous."

"The two sites are very different, though both are at a comparable depth, 60 to 100 feet beneath the ocean's surface. The Yonaguni site might be ceremonial platforms, and the Okinawa site seems similar to a castle wall, a conjecture that is supported by nearby castles on the island with a similar architectural style," says Arbuthnot.

Arbuthnot says that when he came up after the first dive, at Yonaguni, he found little to suggest that it was man made. It was only after diving the Okinawa site, and interviewing Prof. Kimura for



two days, that he began to entertain the notion. The conversations with Prof. Kimura were all the more productive and in-depth, with the translating skills of Corina Tettinger, who speaks fluent Japanese. "The case for the sites being artificial, or modified by man, requires supporting evidence," he says, and "we found very precise rectilinear stone features that seem to be indicative of either artificial tooling, or modifying the natural geology." A particularly intriguing find: holes in the rock platforms. Could these be post holes to support a wooden structure? The terraces are massive, by human standards. But we can imagine naturally terraced platforms easily utilized for ceremonial purposes with the addition of wooden structures built atop them. You'd simply need to insert the supporting beams into the rock, by drilling a few holes.

"What we were able to observe was fascinating and warrants additional research," he says. "There is some false information on the sites out there. We want to bring clarity to the situation, and intend to mount a full-scale scientific expedition to do further investigation." We'll report new developments on this project as they happen.

Geologist Robert Schoch and Egyptologist John Anthony West (both featured in the NBC documentary "The Mystery of the Sphinx") dove many months ago at Yonaguni, also without arriving at any conclusions, only more questions. Schoch focused on determining what geological forces might have been at work here. While he notes that the strong currents might have cut the terraces out of the layered sediments, he has not ruled out human modification. Schoch says he very much wants to go back to dive again before arriving at any conclusions. "I have not seen the other sites," he says, "and, not having previous diving experience, I spent much of my time underwater just staying alive."

We thank Shun Daichi for his help in preparing our report. We thank Shun and Kenichi Shindo, for use of their photos.

Link to: [Link to:](#)  
[Original Set of Six Photos](#) [New Set of Ten Photos](#)

Photos by kind permission of Shun Daichi and Mr. Kenichi Shindo.  
Unauthorized use is strictly prohibited.  
Links by permission only.

My friend Shun Daichi sent these photos along with a video of the extensive coverage given these mystery structures by Japanese TV. The first group of six photos was taken by Mr. Kenichi Shindo. Shun writes, "There are six places they found structures in the Okinawa

area. One is located in Taiwan. All of them are under the sea. All the geologists agree that the underwater structures are at least 12,000 years old."

Among the first Americans to dive the multi-terraced site of the tiny island of Yonaguni were John Anthony West and Geologist Robert Schoch (who in "Mystery of the Sphinx" persuasively argue that The Sphinx must have been carved from a solid outcropping of sandstone at least 8-10,000 years ago, because that's how long it was that the Sahara Desert last received torrential rainfall, and that is the only explanation for the deep vertical weathering on the flanks and enclosure of the Sphinx). In their preliminary examination of just one site, they found a possible geological explanation: it is the natural erosion of the layered sedimentary rock. The strong ocean currents shear it away, "like large chunks of layer cake," leaving what looks like perfectly parallel and finely cut channels and terraces.

At a similar site, the rock below the surface shows similar layering, while the same rock above the surface does not, suggesting it is the wave and water action cutting the rock. Schoch notes that the corrals covering the rock makes it look smoother than it is close up. On the other hand, there are smaller, above surface tombs that are definitely manmade, and look as though their design is derived from the mystery structures, which were above sea level, on the shore, 8 to 10,000 years ago, before inundation by the rising sea levels (some 300 feet) at the end of the last Ice Age, as the glaciers melted. And Schoch says he cannot rule out that these sites were not used or modified by man, and he is eager for further examination.

Shun Daichi, in an Oct 18, 1997 fax, writes: "In Japan Prof. Kimura of Ryukyu University is quite certain that the monument is manmade. Prof. Ishii of Tokyo University, a geologist, feels the under water structure could be manmade. Media in Japan generally show two views but is inclined to the manmade view."

John West tells me that while this may or may not be evidence of a Lost Civilization he feels certain existed, the Pacific Rim and the Mediterranean, just along the continental shelf, are the places to be looking.

| 184|2002-11-07 21:44:44|TTT UUU|Re: Underwater Megaliths off Japan|

I searched some of the websites with info on Yonagui Island (Okinawa chain) and one photographer has reported his photo being doctored by a tourism group (tampered with, computer enhancement creating a giant head relief in stone). The truth will finally come out.

*jojomalig* wrote:

Stone Monuments or Natural Geology?  
Japan's Mysterious Submerged Stone Structures:

New evidence suggests they may have been used by Man  
by Laura Lee

Fotos: <http://www.lauralee.com/japan/japan1.htm>  
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It was treacherous terrain even for experienced divers. "The Yonaguni site is fairly near the shore, so there was heavy surge (the up and down motion of waves) as well as swift currents, and sharks," says Arbuthnot. "On the upside, the area has the third clearest water in the world, with visibility to 200 feet. And the corals were gorgeous."

"The two sites are very different, though both are at a comparable depth, 60 to 100 feet beneath the ocean's surface. The Yonaguni site might be ceremonial platforms, and the Okinawa site seems similar to a castle wall, a conjecture that is supported by nearby castles on the island with a similar architectural style," says Arbuthnot.

Arbuthnot says that when he came up after the first dive, at Yonaguni, he found little to suggest that it was man made. It was only after diving the Okinawa site, and interviewing Prof. Kimura for two days, that he began to entertain the notion. The conversations with Prof. Kimura were all the more productive and in-depth, with the translating skills of Corina Tettinger, who speaks fluent Japanese. "The case for the sites being artificial, or modified by man, requires supporting evidence," he says, and "we found very precise rectilinear stone features that seem to be indicative of either artificial tooling, or modifying the natural geology." A particularly intriguing find: holes in the rock platforms. Could these be post holes to support a wooden structure? The terraces are massive, by human standards. But we can imagine naturally terraced platforms easily utilized for ceremonial purposes with the addition

of wooden structures built atop them. You'd simply need to insert the supporting beams into the rock, by drilling a few holes.

"What we were able to observe was fascinating and warrants additional research," he says. "There is some false information on the sites out there. We want to bring clarity to the situation, and intend to mount a full-scale scientific expedition to do further investigation." We'll report new developments on this project as they happen.

Geologist Robert Schoch and Egyptologist John Anthony West (both featured in the NBC documentary "The Mystery of the Sphinx") dove many months ago at Yonaguni, also without arriving at any conclusions, only more questions. Schoch focused on determining what geological forces might have been at work here. While he notes that the strong currents might have cut the terraces out of the layered sediments, he has not ruled out human modification. Schoch says he very much wants to go back to dive again before arriving at any conclusions. "I have not seen the other sites," he says, "and, not having previous diving experience, I spent much of my time underwater just staying alive."

We thank Shun Daichi for his help in preparing our report. We thank Shun and Kenichi Shindo, for use of their photos.

Link to: Link to:  
Original Set of Six Photos New Set of Ten Photos

Photos by kind permission of Shun Daichi and Mr. Kenichi Shindo.  
Unauthorized use is strictly prohibited.  
Links by permission only.

My friend Shun Daichi sent these photos along with a video of the extensive coverage given these mystery structures by Japanese TV. The first group of six photos was taken by Mr. Kenichi Shindo. Shun writes, "There are six places they found structures in the Okinawa area. One is located in Taiwan. All of them are under the sea. All the geologists agree that the underwater structures are at least 12,000 years old."

Among the first Americans to dive the multi-terraced site of the tiny island of Yonaguni were John Anthony West and Geologist Robert Schoch (who in "Mystery of the Sphinx" persuasively argue that The Sphinx must have been carved from a solid outcropping of sandstone at least 8-10,000 years ago, because that's how long ago it was that the Sahara Desert last received torrential rainfall, and that is the only

explanation for the deep vertical weathering on the flanks and enclosure of the Sphinx). In their preliminary examination of just one site, they found a possible geological explanation: it is the natural erosion of the layered sedimentary rock. The strong ocean currents shear it away, "like large chunks of layer cake," leaving what looks like perfectly parallel and finely cut channels and terraces.

At a similar site, the rock below the surface shows similar layering, while the same rock above the surface does not, suggesting it is the wave and water action cutting the rock. Schoch notes that the corrals covering the rock makes it look smoother than it is close up. On the other hand, there are smaller, above surface tombs that are definitely manmade, and look as though their design is derived from the mystery structures, which were above sea level, on the shore, 8 to 10,000 years ago, before inundation by the rising sea levels (some 300 feet) at the end of the last Ice Age, as the glaciers melted. And Schoch says he cannot rule out that these sites were not used or modified by man, and he is eager for further examination.

Shun Daichi, in an Oct 18, 1997 fax, writes: "In Japan Prof. Kimura of Ryukyu University is quite certain that the monument is manmade. Prof. Ishii of Tokyo University, a geologist, feels the under water structure could be manmade. Media in Japan generally show two views but is inclined to the manmade view."

John West tells me that while this may or may not be evidence of a Lost Civilization he feels certain existed, the Pacific Rim and the Mediterranean, just along the continental shelf, are the places to be looking.

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| 185|2002-11-08 03:21:38|Jojo Malig|Re: Digest Number 75|

perhaps. but what of the giant geometric blocks?

jojo

Message: 2

Date: Fri, 8 Nov 2002 00:44:43 -0500 (EST)

From: TTT UUU

Subject: Re: Underwater Megaliths off Japan

I searched some of the websites with info on Yonagui Island (Okinawa chain) and one photographer has reported his photo being doctored by a tourism group (tampered with, computer enhancement creating a giant head relief in stone). The truth will finally come out. jojomalig wrote: Stone Monuments or Natural Geology?

"Most people are other people - their lives a mimicry, their passions, a quotation." - Oscar Wilde

<http://www.cafeshops.com/twenty something>

<http://twenty something.crimsonblog.com>

<http://twenty twenty.blogspot.com/>

<http://pampangaonline.com>

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| 186|2002-11-08 05:30:26|a.manansala@attbi.com|Re: Digest Number 75|

I remember the discussions about these structures some years ago. What I found unconvincing about the skeptic's arguments was the other examples of ocean shearing they gave really weren't comparable to the Yonaguni structures. Also, why the close coincidence between the known man-made tombs on Okinawa and those formations underwater?

OTOH, no artifacts have yet been discovered associated with the structures. That may be because of the currents in the region and I doubt if any underwater digging has been conducted.

Here is another site on Yonaguni. The text is in Japanese, but the photos are very good and revealing.

<http://www.page.sannet.ne.jp/tsuzuki/yonaguni/yonaguni2.html>

Regards,  
Paul Kekai Manansala

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> Date: Fri, 8 Nov 2002 00:44:43 -0500 (EST)  
> From: TTT UUU  
> Subject: Re: Underwater Megaliths off Japan  
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> <http://twentytwenty.blogspot.com/>  
> <http://pampangonline.com>  
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> Do you Yahoo!?

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| 187|2002-11-08 09:29:46|jojomalig|Re: megaliths|  
there are underwater megaliths off india too, i believe.

Jojo

--- In austrie@y..., a.manansala@a... wrote:  
> I remember the discussions about these structures some years ago.  
What I found  
> unconvincing about the skeptic's arguments was the other examples  
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>  
> Regards,  
> Paul Kekai Manansala  
> >  
| 188|2002-11-08 09:35:58|jojomalig|Bangungot (Sudden Unexplained Nocturnal Death Syndrome )|  
Paul,  
I was researching material on sleep paralysis, a common sleep disorder when one can't move his body in the state of being half-awake and half-asleep when I came across the following article at <http://www.geocities.com/Paris/LeftBank/1700/sunds.html>.  
  
What struck me was that the most common victims of bangungot (Sudden Unexplained Nocturnal Death Syndrome) are Austronesians - Filipinos, Hawaiians, Thais, Laotians, Khmer and other East Asians.  
Could it be the SUNDS/bangungot is a genetically-transferred disorder particularly among Austrics?  
  
Regards,  
Jojo Malig

## Brief History of SUNDS

Sudden Unexplained Nocturnal Death Syndrome (SUNDS) was first diagnosed in the mid-50ies. At first it was thought to be related to Sudden Infant Death Syndrome (SIDS). Four decades later and millions of dollars and hours of research have uncovered surprisingly little about the causes of SUNDS.

Medical examiners in Hawaii were one of the first to make public their findings about SUNDS. In March 1955, a medical examiner, Dr. Nils P. Larsen published twenty one case histories of native Hawaiians who had died from what he termed Sudden Unexplained Death Syndrome. Newspapers, magazines and tabloids soon ran sensational articles about "Sleeping death". At the time Dr. Larsen believed SUDS was a unique, (and possibly genetic) phenomena among the native Hawaiian population.

Not much research was conducted between the 50ies and the early 80ies. The only major research done during this period was conducted in Japan. The results of the Japanese research isolated the phenomena to the minority population of the Caucasoid Ainu in Japan. The researchers believed that cultural stress (due to governmental efforts towards assimilation) was cause of the unexplained deaths.

In the late 70ies and the early 80ies, there was a flurry of international research on the topic of SUNDS. The U.S. experienced an influx of non-literate immigrants from South-East Asia. These tribal people had been recruited from the mountains of Laos and Cambodia to aid the American effort in Vietnam. After the war public and private institutions aided the immigration of these tribal people. Those who couldn't make it to America fled to Thailand where they were kept in large refugee camps.

There were hundrends of cases of SUNDS among these new immigrants from South-East Asia. Local doctors and medical examiners were at a loss as to the reasons of the premature death of healthy young men. Various task forces were initiated both publicly and privately. The largest and most well financed was the task force set up by the auspices of the World Health Organization. In the mid-80ies, there were at least a dozen research organizations studying the problem of SUNDS among South-East Asians. They studied immigrants in America and in Thailand (where SUNDS had become almost epidemic with hundreds of cases reported).

Though no cause was discovered after this decade of intense research, many promising hypotheses were discarded. These included: hereditary, nutritional, toxins, chemical warfare, pollution, and even murder. In the end most of the task forces (including the World Health Organization) gave up in their search for the cause and a cure for SUNDS.

Today few researchers study the puzzling sleep disorder that leads to death. However new research seems to link SUNDS to Sleep Paralysis. The Purpose of this Web Page is to insure further research in this area. For those intersted in finding out more about the history of this research and specific arial suties, please consult the bibliography.

## Characteristics of SUNDS

The research does agree on what are the common characteristics of a Sudden Unexplained Nocturnal Death Syndrome even if its causes remain uncertain.

- 1) SUNDS almost always affects males (99% of all cases have been males).
- 2) SUNDS affects young healthy men (+80% of the victims have been between the ages of 22 to 45. -15% had any medical record of previous cardiological impairment and/or familial record of severe cardiological impairment).
- 3) The death is a result of a miocardial infraction, a heart attack, in the lower left ventricle of the heart.
- 4) Bodies are found in the supine position, on their back, in bed.
- 5) Death usually occurs in the first third of the sleep cycle.
- 6) There appears to be little or no movement or struggle in the dying process of SUNDS (over 50% of the cases died while in bed with their partner and not waking them).
- 7) Many cases are reported to have a fixed and "terrified" expression

on their face.

## SUNDS miscellania

Despite earlier area/population specific research, SUNDS is a world wide phenomena, from Maui to Mexico City, from Montreal to Manchester, and Mombasa to Mongolia. There is no census on how many cases of SUNDS there are every year, but from my research SUNDS probably runs into the thousands. Unfortunately and undersandably SUNDS is often misdiagnosed, especially in developing countries.

Just as SUNDS is a world wide phenomena, it also seems to have been with man since at least the time of Hellenic Greece. Galen wrote about a case that very well could have been a SUNDS. It is perhaps possible that the accounts of Vampire deaths, Succubi deaths and Witchcraft may have actually been cases of SUNDS. Japanese researchers have uncovered good medical cases of SUNDS reaching back to 1920ies. SUNDS is a very old phenomena.

Most cultures have folkloric explanations for SUNDS. Usually the explantion is either witchcraft or the breaking of some cultural taboo. Among the Mong of South-East Asia, the belief is that SUNDS is the result of angry ancestral ghosts. In many parts of the Caribbean "nightmare death" is attributed to bats. In Japan often spectral foxes are considered the culprit. In the West Counties of Ireland, fairies are believed to have killed the victims. The Inuit Eskimo believe the death is caused by the "Snow Witch". The list goes on...It seems most cultures have some sort of explanation for these mysterious deaths. Often these explanations are similar or identical to the explanations for sleep paralysis.

| 189|2002-11-08 16:24:57|TTT UUU|Re: Yonagui megaliths|

Giant geometric blocks? That is not so special really. Crystals form straight linegeometry without human input.Basalt fractures in straight lines. I have seen similar flat platforms and sheared ledgefeatures in Newfoundland and the hexagonal blocks of the Giant's Causeway in Northern Ireland are known from other sites in the world. A professional experienced geologist will have something betterto add than me.

Would like to see the post holes mentioned. Alongrivers over bedrocka feature called apothole is often formed when the current swirls stones in a depression over a long time. These potholes

look man-made, but are not, and can be a meter or more deep. I have seen excellent examples in northern Maine with mice and chipmunks dead in the bottom where they had fallen and could not escape.

Pity this group has no input from Japanese archaeologists in the know. The Yonaguni underwater features were a sensation in magazines and TV.

I'd like to know more about Indian megaliths. Have a book coming about Indonesian stone sculpture in German museums. Should be interesting. Will share.

***a.manansala@attbi.com*** wrote:

I remember the discussions about these structures some years ago. What I found unconvincing about the skeptic's arguments was the other examples of ocean shearing they gave really weren't comparable to the Yonaguni structures. Also, why the close coincidence between the known man-made tombs on Okinawa and those formations underwater?

OTOH, no artifacts have yet been discovered associated with the structures. That may be because of the currents in the region and I doubt if any underwater digging has been conducted.

Here is another site on Yonaguni. The text is in Japanese, but the photos are very good and revealing.

<http://www.page.sannet.ne.jp/tsuzuki/yonaguni/yonaguni2.html>

Regards,  
Paul Kekai Manansala

>

> perhaps. but what of the giant geometric blocks?

> jojo

> Message: 2

> Date: Fri, 8 Nov 2002 00:44:43 -0500 (EST)

> From: TTT UUU

> Subject: Re: Underwater Megaliths off Japan

>

>

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| 190|2002-11-08 16:55:31|TTT UUU|Undersea pre-Harappan (7,500BP) City, NW India.|

Just found the BBCnews of the undersea pre-Harappan (approx. 7,500 BP)city off the NW coast of India, Gulph of Cambaystate of Gujarat, near Pakistan.  
[http://news.bbc.co.uk/1/hi/world/south\\_asia/1768109.stm](http://news.bbc.co.uk/1/hi/world/south_asia/1768109.stm)

*jojomalig* wrote:

there are underwater megaliths off india too, i believe.

Jojo

--- In austric@y..., a.manansala@a... wrote:

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What I found

> unconvincing about the skeptic's arguments was the other examples of ocean

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> Paul Kekai Manansala

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| 191|2002-11-08 20:53:17|a.manansala@attbi.com|Re: Yonagui megaliths|

- >
- > Giant geometric blocks? That is not so special really. Crystals form straight
- > line geometry without human input. Basalt fractures in straight lines. I have
- > seen similar flat platforms and sheared ledge features in Newfoundland and the
- > hexagonal blocks of the Giant's Causeway in Northern Ireland are known from
- > other sites in the world.

Could you share some pictures of these formations? I remember when that earlier "debunking" photos were not very convincing. They were not comparable to the Yonaguni structures.

Although I'm not generally a fan of Graham Hancock he also has some interesting photos on his site and compares the stone Yonaguni terrances with rock-hewn terrances in the Andes. I've seen similar structures in Polynesia that served as foundations for villages.

<http://www.grahamhancock.com/gallery/underwater/yonaguni.htm>

Regards,

Paul Kekai Manansala

| 192|2002-11-09 00:26:03|jojomalig|Assistant Curator of Southeast Asian Art|

From: "Elrik Jundis"

Date: Fri Nov 8, 2002 11:57 pm

Subject: Assistant Curator of Southeast Asian Art

Assistant Curator of Southeast Asian Art

-----  
-----  
----

Reply to: [www.asianart.org](http://www.asianart.org)

Date: Wed Nov 6th 04:43

The Asian Art Museum offers an exciting opportunity for a motivated and resourceful scholar in Southeast Asian art to join the curatorial staff of the New Asian at this historic time. This is a part-time position for 20 hours per week.



With its state-of-the-art galleries, the New Asian will be one of the cultural crown jewels of the Bay Area, and a powerful magnet for art lovers

from around the world. The museum's collection of Southeast Asian art consists of 1500 objects representing the art and culture of Thailand, Cambodia, Burma, Vietnam, Indonesia, and the Philippines.

The preferred candidate should be a specialist in the arts of Indonesia. The

museum's collection of Indonesian art spans from 800 to the present including central and east Javanese bronze and stone sculpture and decorative arts, 18th through 20th century Balinese arts, and important groups of krises and Wayang Golek puppets.

The Assistant Curator will perform a variety of curatorial duties related to

the identification, acquisition, preservation, exhibition, and interpretation of objects in the museum's Southeast Asian Art collection including:

- ? assisting in the organization of exhibitions (may be assigned to independently organize smaller, less complicated exhibitions);

- ? conducting research; preparing interpretive and informational labels and brochures; writing essays and articles as required; preparing and presenting lectures and gallery tours.

- ? assisting in planning and organizing educational programs in conjunction with Education Department staff; providing training for docents as required.

- ? investigating and recommending with regard to potential donations.

- ? assisting in fundraising and public relations efforts related to departmental activities; assisting with cultivation of donors and collectors.

## MINIMUM QUALIFICATIONS

- ? M.A. in Asian Art History, with field of interest in Southeast

Asian Art;

Ph.D. in Asian Art History preferred. Specialization in arts of Indonesia

highly desirable.

? One year of curatorial or related experience; or an equivalent combination

of education, training, and experience.

? Solid research skills: must be able to interpret the collections and to

communicate knowledge relevant to the collection.

? Superb writing skills; ability to write effectively for both general and scholarly audiences.

? Good computer skills; knowledge of Microsoft Word.

? Ability to conduct original research; reading knowledge of Dutch and/or

Indonesian preferred.

? Ability to address general public audiences in writing and speaking.

? Ability to work independently and to exercise initiative and good judgment; extremely reliable and dependable.

? Ability to establish and maintain positive and effective working relationships with staff, visitors, volunteers, and representatives of other institutions.

? Detail-oriented, meticulous, and conscientious in following through on assigned tasks.

## COMPENSATION

\$19.06-22.89/hr.

with excellent benefits package\*

\*Please note: 1. New hires start at the beginning of the salary range

2. This

position is represented by SEIU 790

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Apply online at [www.asianart.org](http://www.asianart.org) or

Send a letter of interest and resume as soon as possible to:

HUMAN RESOURCES

Asian Art Museum

Golden Gate Park

San Francisco, CA 94118  
FAX: 415.379.8887

\*The Asian Art Museum, a premier San Francisco institution, is one of the largest museums in the Western world devoted exclusively to Asian art. The Museum's magnificent and priceless collection of more than 13,000 objects, including paintings, sculpture, ceramics, bronzes, jades and textiles, exemplifies the artistic accomplishments of countries and cultures throughout Asia. The museum is scheduled to relocate in 2002 to an historic Civic Center building which is currently undergoing a \$160 million dollar renovation to become the Asian Art Museum's new home. The museum will open to the public in early 2003.\*

The Asian Art Museum is proud to be an Equal Opportunity Employer.

Other ways to contact poster:  
F: 415-379-8887

This is in or around Civic Center.  
This is at a non-profit organization.

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No phone calls, please!  
Contacting the poster about other services, products or commercial interests is NOT OK.  
Reposting this message elsewhere is NOT OK.  
| 193|2002-11-11 18:56:41|Paul Kekai Manansala|Re: Yonagui megaliths|  
Here are some photos of Maori "pa" sites.

[http://www.nzarchaeology.org/index\\_files/pa.jpg](http://www.nzarchaeology.org/index_files/pa.jpg)  
<http://www.aucklandcity.govt.nz/dbtw-wpd/heritageimages/images/3f/img0098.jpg>

You can find pics of the similar Marquesan tohua in Bellwood's  
\_Man's Conquest of the Pacific\_.

Regards,  
Paul Kekai Manansala  
| 194|2002-11-11 20:14:09|TTT UUU|Re: Yonagui megaliths|

By "pa" do you mean sculpted earthworks as fortified hill tops? They are present in Micronesia as well. Will cite the book source later (at home).

Robin Day

***Paul Kekai Manansala*** wrote:

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[http://www.nzarchaeology.org/index\\_files/pa.jpg](http://www.nzarchaeology.org/index_files/pa.jpg)  
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| 195|2002-11-11 20:37:08|TTT UUU|Re: Yonagui megaliths|

The Giant's Causway in Northern Ireland is an internationally known site showing basalt hexagons. A quick web search will provide photos.

The stone blocks and ledges I saw in eastern Newfoundland .....well that goes back to when I was 10 or 11 years old. I have no photos. The place was called Englewood near Clarendville.

The photos from Graham Hancock's webpage are very good Paul but I am unconvinced. My (botanist) opinion of these photos is meaningless as is Hancock's. A qualified geological report would be taken seriously. If anything these stone steps seem haphazard and do not look like any man-made structures I have seen anywhere. Did Hancock not get the stone tested and identified? Is it basalt? Seems that would be an obvious first step. What are the dimensions of these features? His books are a bit slap-dab as we both know.

I have seen the steps and platforms cut into the solid rock of Machu Pichu (second-hand from books and TV). There is some resemblance here. I am not a skeptic, just wanting to know more.

Robin Day

*a.manansala@attbi.com* wrote:

>  
> Giant geometric blocks? That is not so special really. Crystals form straight  
> line geometry without human input. Basalt fractures in straight lines. I have  
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<http://www.grahamhancock.com/gallery/underwater/yonaguni.htm>

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| 196|2002-11-11 21:49:47|a.manansala@attbi.com|Re: Yonagui megaliths|  
Yes, "pa" refers to a fortified settlement.

Regards,  
Paul Kekai Manansala

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> By "pa" do you mean sculpted earthworks as fortified hill tops? They are  
> present in Micronesia as well. Will cite the book source later (at home).  
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> <http://www.aucklandcity.govt.nz/dbtw->  
> [wpd/heritageimages/images/3f/img0098.jpg](http://www.aucklandcity.govt.nz/dbtw-wpd/heritageimages/images/3f/img0098.jpg)  
>  
>  
> You can find pics of the similar Marquesan tohua in Bellwood's  
> \_Man's Conquest of the Pacific\_.  
>  
> Regards,  
> Paul Kekai Manansala  
>  
>  
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| 197|2002-11-12 17:50:26|TTT UUU|Re: Yonagui megaliths|

Childress, DavidHatcher 1998. Ancient Micronesia & the lost city of Nan Madol. Available through Amazon.com

This is the book that shows fortified hill tops "pa" in Micronesia (Mt. Eden, Truk Islands, Babeldaob)and numerous examples of megaliths throughout the Pacific including the rectangularstep-pyramid on Tahiti and pilars on Guam.

Childressis a questionable amateur archaeologist (like Hatcher and Eric Von Daniker ..Chariots of the Gods, etc.)but he does collect together a lot of interesting material, especially photos.His book (p.185)also shows another grainy underwaterphoto of Yonaguni Island, Okinawa,this one from an article in Super Mystery Magazine, Tokyo (edition not given).

Childress provides a lot of interesting material about Australia and possible ancient trade contactin his last chapter. This is not generally seen elsewhere.

Robin Day

***a.manansala@attbi.com*** wrote:

Yes, "pa" refers to a fortified settlement.

Regards,  
Paul Kekai Manansala

>

> By "pa" do you mean sculpted earthworks as fortified hill tops? They are

> present in Micronesia as well. Will cite the book source later (at home).

> Robin Day

> Paul Kekai Manansala wrote:Here are some photos of

> Maori "pa" sites.

>

> [http://www.nzarchaeology.org/index\\_files/pa.jpg](http://www.nzarchaeology.org/index_files/pa.jpg)

> <http://www.aucklandcity.govt.nz/dbtw->

> [wpd/heritageimages/images/3f/img0098.jpg](http://www.aucklandcity.govt.nz/dbtw-wpd/heritageimages/images/3f/img0098.jpg)

>

>

> You can find pics of the similar Marquesan tohua in Bellwood's

> \_Man's Conquest of the Pacific\_.

>

> Regards,

> Paul Kekai Manansala

>

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| 198|2002-11-12 18:08:21|TTT UUU|Re: Genetics article Africa, Greece, Genetic Atlas|

The northAfrican influence in the Greek genome is interesting. Many art historians also see a huge Egyptian influence in Greek art and architecture. The Greeks had a colony at Cyrene city on the coast, west of Alexandria and the Ptolemy Kings were in power for only about 300 years in Egypt. The African genetic inputs may be primarily Minoan-Crete (rather than northern Greek-Agamemnon derived) or very much older. I have read the human genome atlas put out by Luigi Luca Cavalli-Sforza and his group (Stanford U.). It was published in 1995 or 96. There have been many advances since then. I have not kept up.

Robin Day S. Korea

*Paul Kekai Manansala* wrote:



The following abstract is interesting with regard to recent discussion of the "Aryan invasion."

Regards,  
Paul Kekai Manansala

---

Tissue Antigens 2002 Aug;60(2):111-21

Population genetic relationships between Mediterranean populations determined by HLA allele distribution and a historic perspective.

Arnaiz-Villena A, Gomez-Casado E, Martinez-Laso J.

HLA genes allele distribution has been studied in Mediterranean and sub-Saharan populations. Their relatedness has been tested by genetic distances, neighbour-joining dendrograms and correspondence analyses. The population genetic relationships have been compared with the history of the classical populations living in the area. A revision of the historic postulates would have to be undertaken, particularly in the cases when genetics and history are overtly discordant. HLA genomics shows that: 1) Greeks share an important part of their genetic pool with sub-Saharan Africans (Ethiopians and west Africans) also supported by Chr 7 Markers. The gene flow from Black Africa to Greece may have occurred in Pharaonic times or when Saharan people emigrated after the present hyperarid conditions were established (5000 years B.C.). 2) Turks (Anatolians) do not significantly differ from other Mediterraneans, indicating that while the Asians Turks carried out an invasion with cultural significance (language), it is not genetically detectable. 3) Kurds and Armenians are genetically very close to Turks and other Middle East populations. 4) There is no HLA genetic trace of the so called Aryan invasion, which has only been defined on doubtful linguistic bases. 5) Iberians, including Basques, are related to north-African Berbers. 6) Present-day Algerian and Moroccan urban and country people show an indistinguishable Berber HLA profile.

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| 199|2002-11-13 17:10:03|TTT UUU|Nusantarian (Austrie) World|

[www.lemurdolls.com/manusa.htm](http://www.lemurdolls.com/manusa.htm)

Interesting reading and refs about the sea-going austrie culture groups.

---

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| 200|2002-11-13 21:58:15|jojomalig|Re: Nusantarian (Austrie) World|

This is from a Malayo perspective, if I am correct.

I have a deep knowledge of Austronesian studies, but I am very unfamiliar with Nusantarian. Any take on this, Chris Sundita?

Mentioned: "(Pekan, for the Nusantarian natives), the archipelago of Hawaii (from "Hava-iki" or "Little Java", to recollect the ancestral homeland of the Polynesian), and New Zealand (Aotearoa in Maori language). Beyond this heartland, other regions were frequented by Nusantarians navigators, including the major part of the Pacific Ocean (to South America) and the Indonesian Ocean, as far as East Africa."

From what source are the placenames Pekan and Hava-iki? I have already read many times over in mainstream austronesian literature that the Philippines is the motherland of present-day Hawaiians and various peoples in Oceania, Polynesia and Micronesia, and the Indo-Malay islands as the Philippine archipelago was the focal point of the Austronesian expansion from the tip of present-day Taiwan.

Hawaii=Hava-iki=Little Java?

regards,  
Jojo Malig

Jojo

What s

--- In austric@y..., TTT UUU wrote:

>

> [www.lemurdolls.com/manusa.htm](http://www.lemurdolls.com/manusa.htm)

>

> Interesting reading and refs about the sea-going austric culture groups.

>

>

>

> -----

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| 201|2002-11-14 12:03:13|a.manansala@attbi.com|Re: Nusantaraian (Austric) World|

> This is from a Malayo perspective, if I am correct.

>

> I have a deep knowledge of Austronesian studies, but I am very

> unfamiliar with Nusantaraian. Any take on this, Chris Sundita?

>

Nusantara comes from Indonesian. I guess the connection is with the phrase "Nusantao" coined by Wilhelm Solheim as an a term meaning "Austronesian" but with indigenous roots.

>

> From what source are the placenames Pekan and Hava-iki? I have  
> already read many times over in mainstream austronesian literature  
> that the Philippines is the motherland of presend-day Hawaiians and  
> various peoples in Oceania, Polynesia and Micronesia, and the Indo-  
> Malay islands as the Philippine archipelago was the focal point of  
> the Austronesian expansion from the tip of present-day Taiwan.

>

> Hawaii=Hava-iki=Little Java?

>

Hawaiiki comes from Maori and is the same word as Hawai`i and Savai`i (Samoan).

Some believe the "hawa" or "sava" refers to geographical terms like "Sabah" and "Java" further east.

The Philippines is believed by some to be the launching point for the Pacific migrations.

Solheim believes the southern Philippines or eastern Indonesia was the birthplace of Austronesian, in contrast to Blust and Bellwood who argue for a Taiwan homeland.

Regards,  
Paul Kekai Manansala  
| 202|2002-11-14 13:01:31|a.manansala@attbi.com|Re: Nusantaraian (Austic) World|  
Btw, "nusan-tao" means "island people."

Regards,  
Paul Kekai Manansala  
| 203|2002-11-14 22:30:53|jojomalig|Re: Nusantaraian (Austic) World|  
thanks.  
jojo malig

--- In austic@y..., a.manansala@a... wrote:

>

> Btw, "nusan-tao" means "island people."

>

> Regards,

> Paul Kekai Manansala

| 204|2002-11-15 07:38:17|Richard Wordingham|Origin of Tai Voiceless Aspirates|

Does anyone know whether the voiceless aspirates of proto-Tai go back to Austric? I recently cited the infrequency of their reflexes (compared to those of unaspirated voiceless, voiced and pre-glottalised plosives) in Thai as an example of how rare voiceless aspirates can be in a system with several other plosive phonations, but I am wondering if this is a valid comparison. Their fewness in number, especially of proto-Tai \*ch, has occasionally made me wonder if there were something peculiar about them. In the modern 3-way system of Thai, voiceless aspirates are numerous, but most of them, if ancient, derive from the proto-Tai voiced plosives.

Richard.  
| 205|2002-11-15 14:05:05|a.manansala@attbi.com|Re: Origin of Tai Voiceless Aspirates|  
Richard,

Do you mean Austro-Tai, or do you include Tai as part of the Austric family.

That would not be unusual because there are a lot of different theories regarding Austric these days.

Or maybe you are suggesting borrowing from Austric?

Languages in Mon-Khmer and Munda have voiceless aspirates although Hayes doesn't have them in his reconstruction of Proto-Austroasiatic phonology.

Regards,

Paul Kekai Manansala

> Does anyone know whether the voiceless aspirates of proto-Tai go back  
> to Austric? I recently cited the infrequency of their reflexes  
> (compared to those of unaspirated voiceless, voiced and pre-  
> glottalised plosives) in Thai as an example of how rare voiceless  
> aspirates can be in a system with several other plosive phonations,  
> but I am wondering if this is a valid comparison. Their fewness in  
> number, especially of proto-Tai \*ch, has occasionally made me wonder  
> if there were something peculiar about them. In the modern 3-way  
> system of Thai, voiceless aspirates are numerous, but most of them,  
> if ancient, derive from the proto-Tai voiced plosives.

>

> Richard.

>

>

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| 206|2002-11-17 02:48:17|LV Hayes|Re: Origin of Tai Voiceless Aspirates|

>From: [a.manansala@attbi.com](mailto:a.manansala@attbi.com)

>Date: Fri, 15 Nov 2002 21:54:01 +0000

>Subject: Re: [austric] Origin of Tai Voiceless Aspirates

>

>Do you mean Austro-Tai, or do you include Tai as part of the  
>Austric family.

>

>That would not be unusual because there are a lot of  
>different theories regarding Austric these days.

>

>Or maybe you are suggesting borrowing from Austric?

>

>Languages in Mon-Khmer and Munda have voiceless aspirates  
>although Hayes doesn't have them in his reconstruction of  
>Proto-Austroasiatic phonology.

The evidence I've seen dictates the conclusion that aspirated phonemes are secondary developments in, rather than original sounds of, the Austric (Austroasiatic + Austronesian) language family.

Note that Paul Benedict did not reconstruct aspirated phonemes for his Proto-Austro-Tai phonology, cf. table of Austro-Thai Consonants (Oral) on page 155 of \_Austro-Thai Language and Culture with a Glossary of Roots\_, HRAF Press, 1975.

With respect to the Thai/Tai side of things, what does Weera Ostapirat have to say in his PhD dissertation at UC Berkeley on Proto-Kradai (formerly Tai-Kadai)? I haven't been able to contact him (her?) to ask about this and other things.

LV Hayes  
| 207|2002-11-17 17:55:12|TTT UUU|Indonesian Sculptures|

Alain Vairo, author

Messages in Stone: Statues and Sculpture from Tribal Indonesia in the Collection of the Barbier-Mueller Museum. 200pages. 285 illustrations.

Anybody read this book? There is no review on Amazon.com

---

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| 208|2002-11-18 07:51:43|Richard Wordingham|Re: Origin of Tai Voiceless Aspirates|  
--- In austric@y..., LV Hayes wrote:  
> >From: a.manansala@a...  
> >Date: Fri, 15 Nov 2002 21:54:01 +0000  
> >Subject: Re: [austric] Origin of Tai Voiceless Aspirates  
  
> >Or maybe you are suggesting borrowing from Austric?

I wasn't thinking of borrowing. But I would be very interested if there was evidence that a significant proportion of proto-Tai voiceless aspirates were borrowings.

> >Languages in Mon-Khmer and Munda have voiceless aspirates  
> >although Hayes doesn't have them in his reconstruction of  
> >Proto-Austroasiatic phonology.

>

> The evidence I've seen dictates the conclusion that aspirated

phonemes are

> secondary developments in, rather than original sounds of, the

Austroic

> (Austroasiatic + Austronesian) language family.

I had been assuming that (Tai-)Kadai was part of Austroic. Is there clearly a split between Austroasiatic and Austronesian on one hand and Kadai on the other?

> Note that Paul Benedict did not reconstruct aspirated phonemes for

his

> Proto-Austro-Tai phonology, cf. table of Austro-Thai Consonants

(Oral) on

> page 155 of *Austro-Thai Language and Culture with a Glossary of*

*Roots*,

> HRAF Press, 1975.

Alas, I do not have access to this book - at least, not quickly.

However, that indicates either that they are not so very ancient, or that they were not numerous enough to reconstruct.

> With respect to the Thai/Tai side of things, what does Weera

Ostapirat have

> to say in his PhD dissertation at UC Berkeley on Proto-Kradai

(formerly

> Tai-Kadai)? I haven't been able to contact him (her?) to ask about

this

> and other things.

Don't know. 'Comparative Kadai' (pages of which have been scanned in for the Rosetta Project) talks as though Mak once had voiceless aspirates, but I'll have to check the data. I saw a few Mak words in h- in the Rosetta Project database that I think have Thai cognates starting with khor khai (Thai khEEk 'guest', khOO 'neck'), but they

may derive from Proto-Tai \*\*x. I'll have to check. My Thai spelling's lousy.

Richard.

| 209|2002-11-19 02:25:52|Richard Wordingham|Re: Origin of Tai Voiceless Aspirates|  
--- In austrie@y..., "Richard Wordingham"

wrote:

> --- In austrie@y..., LV Hayes wrote:  
> > With respect to the Thai/Tai side of things, what does Weera  
> Ostapirat have  
> > to say in his PhD dissertation at UC Berkeley on Proto-Kradai  
> (formerly  
> > Tai-Kadai)? I haven't been able to contact him (her?) to ask

about

> this  
> > and other things.  
>  
> Don't know. 'Comparative Kadai' (pages of which have been scanned

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> for the Rosetta Project) talks as though Mak once had voiceless  
> aspirates, but I'll have to check the data. I saw a few Mak words

in

> h- in the Rosetta Project database that I think have Thai cognates  
> starting with khor khai (Thai khEEk 'guest', khOO 'neck'), but they  
> may derive from Proto-Tai \*\*x. I'll have to check. My Thai  
> spelling's lousy.

Whoops! khOO is spelt with khor khwaai (from proto-Tai \*G in this word)! And khEEk derives from proto-Tai \*x. However, I did find five Mak words in the database for which Fang-Kuei Li reconstructs proto-Tai voiceless aspirates, namely cognates of proto-Tai \*phlMak A1 'taro', \*khEEEn A1 'arm', \*khraM or \*khlaM B1 'egg', \*thaap D1 'to carry on a pole' and \*khwaa 'right', where [M] is the close back unrounded vowel. It therefore looks as though there ought to be voiceless aspirates in proto-Kadai, or at least in proto-Kam-Tai. However, \*kh, \*x and \*G are a bit of a mess in Li's reconstruction of proto-Tai (White Tai, L and early Sukhothai words in x-, which were spelt using khor khuat, don't fit, as he admits), and the reconstructions may have been improved by now.

Richard.

| 210|2002-11-19 08:03:55|LV Hayes|Re: Origin of Tai Voiceless Aspirates|



>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>  
>Date: Mon, 18 Nov 2002 15:51:39 -0000  
>  
>I had been assuming that (Tai-)Kadai was part of Austric.  
>Is there clearly a split between Austroasiatic and  
>Autstronesian on one hand and Kadai on the other?

As Paul Manansala said, "there are a lot of different theories regarding Austric these days." This situation of great and unnecessary confusion arises primarily from the ignorance or arrogance of Merritt Ruhlen who in 1987 used Wilhelm Schmidt's Austric as the equivalent of Benedict's Austro-Tai. Even people who know better persist in propagating Ruhlen's error. After all, Ruhlen is Stanford, disciple of Greenberg, yah di yah di yah di; what does mere fact mean when fame is at hand?

Schmidt meant Austric to encompass only 2 language families, AA (Austroasiatic) and AN (Austronesian), and presented linguistic evidence for their genetic affiliation. His phonological and morphological evidence was convincing, but not his lexical; hence, Schmidt's Austric has never become well accepted, even after all of my own efforts to make it so.

Benedict used AT (Austro-Tai) to encompass AN, Miao-Yao (AKA Hmong-Mien), and Tai-Kadai (now Kradai?). Note that he came out with this idea a good 20 years before Ruhlen's 1987 book confusing Austric and AT. He specifically rejected the inclusion of AA and tried to include Japanese-Ryukyuan (which many people believe a mistake). He also presented linguistic evidence for all these languages being genetically related, but the current consensus of opinion about his evidence is probably the same granted Schmidt in an earlier day: it ain't necessarily so.

As I've said in my published articles, Austric (AA + AN) may be assumed to belong within the AT grouping on the basis of Benedict's finding that AN belongs therein. I have produced evidence which I think conclusively demonstrates that AA and AN are genetically related (and thus Schmidt's Austric hypothesis is verified), but I have never tried to demonstrate any such genetic relationship between Austric or AA or AN to the other languages.

Thus, Tai-Kadai belongs within AT, if you follow Benedict, or Austric, if you follow Ruhlen. Since Ruhlen doesn't recognize the existence of AT, I suppose it's "a six of one, half a dozen of the other situation" for many observers.

As far as I'm concerned, the question of the type of linguistic relationship existing between Tai-Kadai and AA and/or AN remains to be answered definitively. Hearsay has it that Ostapirat's dissertation has

produced evidence answering or crucial to answering this question, but I have not seen dissertation or evidence or talked to anyone who has. Which isn't atypical for this field of linguistics: finding anything may take you a while.

LV Hayes

| 211|2002-11-19 08:32:40|LV Hayes|Re: Origin of Tai Voiceless Aspirates|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>

>Date: Tue, 19 Nov 2002 10:25:48 -0000

>

>Whoops! khOO is spelt with khor khwaai (from proto-Tai

>\*G in this word)! And khEEk derives from proto-Tai \*x.

>However, I did find five Mak words in the database for

>which Fang-Kuei Li reconstructs proto-Tai voiceless

>aspirates, namely cognates of proto-Tai \*phlMak A1

>'taro', \*khEEn A1 'arm', \*khraM or \*khlaM B1 'egg',

>\*thaap D1 'to carry on a pole' and \*khwaa 'right', where

>[M] is the close back unrounded vowel. It therefore

>looks as though there ought to be voiceless aspirates in

>proto-Kadai, or at least in proto-Kam-Tai. However, \*kh,

>\*x and \*G are a bit of a mess in Li's reconstruction of

>proto-Tai (White Tai, L and early Sukhothai words in x-,

>which were spelt using khor khuat, don't fit, as he

>admits), and the reconstructions may have been improved

>by now.

Yes, and keeping up with the latest is always a problem.

I have Li's book and the \_Comparative Kadai\_ thing, so I know what you're talking about; still, I don't really have the knowledge or resources to talk familiarly or with confidence about Tai and Kadai comparative linguistics.

In Mon-Khmer, the aspirates are mostly the product of 2 types of phonological change. First, \*p > ph or \*b > ph in some languages as an articulatory effect of registogenesis. Second, \*pVh- > ph- or \*bVh > bh, where segmental restructuring is involved. Often, the presyllabic element can be identified as an old prefix. The first type of change has obviously occurred in Tai; I don't know if the second type can be identified in Tai or not.

It would appear that the Austric languages had affixes from an early date, so if Tai-Kadai is indeed related, then one must anticipate that affixes once also occurred in Tai-Kadai and reflexes thereof can or will be identifiable in some context to some degree.

Tai-Kadai has clearly borrowed a great deal from the Chinese, so another consideration is whether or not any stage of Chinese had aspirates and whether or not Tai-Kadai could have borrowed some of these consonants. Again, I have neither knowledge nor resources to answer such questions.

LV Hayes

| 212|2002-11-21 05:54:41|Richard Wordingham|Re: Origin of Tai Voiceless Aspirates|

--- In austriac@y..., LV Hayes wrote:

> >From: "Richard Wordingham"

> >Date: Tue, 19 Nov 2002 10:25:48 -0000

> Yes, and keeping up with the latest is always a problem.

I dug this up in an article \_cached\_ by Google from www.reseau-asie.com (a search on Kra-dai and Ostapirat will get it from www.google.co.uk ; the URL now accesses something different!):

'En ce qui concerne l'Austro-Tai, Schlegel (1901) avait d'abord noté les rapports entre les langues tai et les langues austronésiennes. Mais c'est Benedict qui a véritablement proposé en 1942, l'existence d'une macro-famille Austro-tai, comprenant deux branches : le tai-kadai et l'austronésien, auxquelles on a parfois ajouté le miao-yao. Cette hypothèse est aujourd'hui toujours défendue par Ostapirat (2001) et critiquée entre autres, par Thurgood (1994).'

The reference is 'Ostapirat W. (2001), 'Kra-dai and Austronesian : notes on some phonological correspondences'. Communication au symposium sur les perspectives d'une phylogénie des langues d'Asie Orientale. Pionniers.'

Is the following perchance the catalogue entry for his thesis?

Reference Type:Edited Book

Author:Ostapirat, Weera

Year:1999

Title:Proto-Kra

University:Berkeley

Copies:1

Location:L

Call number:xx

(from <http://www.linguistics.berkeley.edu/Hemloc/HEMLOC.html> )

> In Mon-Khmer, the aspirates are mostly the product of 2 types of  
> phonological change. First, \*p > ph or \*b > ph in some languages

as an

> articulatory effect of registrogenesis. Second, \*pVh- > ph- or

\*bVh > bh,

> where segmental restructuring is involved. Often, the presyllabic

element

> can be identified as an old prefix. The first type of change has

obviously

> occurred in Tai; I don't know if the second type can be identified

in Tai

> or not.

In Tai, the first type of change occurred after proto-Tai, and only yielded aspirates in some dialects. Strangely enough, the only cognate of a Tai voiced plosive I can see in the Mak sample looks like a reflex of proto-Kradai \*ʔg, which seems to have deglottalised to proto-Tai \*g, leaving the series of preglottalised stops incomplete. Maybe I haven't looked hard enough. The proto-Tai affricates aren't well represented either; \*dʒ is the only one I can see a correspondence for.

Have you ever looked and failed with the second type? The frequent change proto-Tai \*tr- > Central Tai dialect th- shows restructuring, and the various words for 'who?' (example in Li) - Siamese 'khrai' from 'khon rai' and, elsewhere, 'phrai' from 'phu: rai' or its reflexes, show that syllables can coalesce. I know very few Austro-Asiatic or Austronesian words.

> It would appear that the Austric languages had affixes from an

early date,

> so if Tai-Kadai is indeed related, then one must anticipate that

affixes

> once also occurred in Tai-Kadai and reflexes thereof can or will be

> identifiable in some context to some degree.

The only hints of vanished prefixes (as opposed to the abundant classificatory prefixes) that I am aware of are:

(1) the voicing alternation that Li discusses at the start of his section on tones. I wouldn't build an Austro-Welsh family on that basis! I suppose the tone alternations

(2) the apparently meaningless prefixes k(r)a- and ta- (in Siamese).

> Tai-Kadai has clearly borrowed a great deal from the Chinese, so

another

> consideration is whether or not any stage of Chinese had aspirates

and

> whether or not Tai-Kadai could have borrowed some of these

consonants.

> Again, I have neither knowledge nor resources to answer such

questions.

Well, Old Chinese had voiceless aspirates and Northern Tai reflexes of three of Li's examples of simple proto-Tai aspirates appear to have been replaced by very similar Chinese loans, so it's certainly possible. The strange thing here is that Northern Tai and, I think, many of the non-Tai Tai-Kadai dialects have deaspirated their aspirates and have now acquired a fresh set from Chinese loans.

Should I take it that the purely Tai-Kadai aspects of this discussion are off-topic?

Richard.

| 213|2002-11-26 09:10:23|austriac@yahoo.com|New file uploaded to austriac |  
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Description : Main Theories on the Peopling of the Philippines

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Regards,

jojomalig <[jojomalig@yahoo.com](mailto:jojomalig@yahoo.com)>

| 214|2002-11-26 10:15:09|a.manansala@attbi.com|Re: New file uploaded to austric|

Jojo,

These look like theories pertaining possibly only to the Austronesian migrations. There already is evidence of human habitation in the Philippines by at least 9000 BCE if my memory serves me correctly.

For sure, there are rock shelter habitations dating from 5,000 to 6,000 BCE. Also, I notice no dates are given for Solheim's postulation.

Regards,

Paul Kekai Manansala

>

> Hello,

>

> This email message is a notification to let you know that

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>

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> Regards,

>

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>

| 215|2002-11-26 11:04:21|a.manansala@attbi.com|Archaeologists announce discovery of  
underwater man-made wall off T|

Notice the article mentions the Yonaguni megaliths.

Regards,

Paul Kekai Manansala

---

Archaeologists announce discovery of underwater man-made wall

2002/11/26

The China Post staff

Underwater archaeologists yesterday announced the discovery of a man-made wall submerged under the waters of the Pescadores Islands that could be at least six and seven thousand years old.

Steve Shieh, the head of the planning committee for the Taiwan Underwater Archaeology Institute, said the wall was discovered to the northwest of Tong-chi Island in the Pescadores towards the end of September.

The stone wall, with an average height of one meter and a width of 50 centimeters, covers a distance of over 100 meters, Hsieh said.

The wall ran along the ocean floor at depths of between 25 and 30 meters, he added.

Shieh said that divers found several places along the wall where holes were apparently filled up with pebbles, possibly in an attempt to block winds.

The wall was located by a team of divers working in cooperation with the National Museum of History and the Department of Environmental Sciences at the National Sun Yat-sen University.

In August, researchers scanning waters in the area with sonar discovered what appeared to be the remnants of four to five man-made walls running along the bottom of the sea.

Please see WALL on page

Despite difficult diving conditions, Shieh said that a team of more than ten specialists was able to ascertain the positions of at least three of the wall sections.

The proximity of the wall to a similar structure found in 1976 suggests that it may be further evidence of a pre-historical civilization.

A three meter high underwater wall was discovered by amateur divers in waters off the nearby Hu-ching (Tiger Well) Island.

British archaeologists examined the find and proclaimed that the wall was probably made between 7,000 and 12,000 years ago.

The current find stands a mere 100 meters from the site of that discovery.

Six years ago, evidence of a sunken city in the area was found when amateur divers found the remains of what appear to be city walls taking the shape of a cross on the ocean floor.

Further examination suggested the ruins were made between seven and ten thousand years ago as well, although Japanese researchers put the walls construction at between 10,000 and 80,000 years ago.

Taken together, the discoveries have helped to overturn the established notion that Taiwan's earliest aboriginal inhabitants made their way here from mainland China some 6,000 years ago.

The underwater finds are part of a growing body of evidence suggesting the existence of civilizations older than anything previously imagined.

On this theory, entire cities ended up underwater after sea levels rose towards the end of the last Ice Age, a date cited by Plato as being some 9,600 years ago.

One of the most dramatic examples of evidence of civilizations found on ocean beds has been megalithic structures off the coast of Yonaguni-jima in Japan that have been interpreted in some circles as being built for sacrificial rites. According to Shieh, a similar structure has been located off of the shores of Taiwan's Pingtung County .

Shieh said that he and his association have plans to explore that location as



well as what appears to be a man-made path on the ocean floor off of Taitung County sometime next year.

| 216|2002-11-26 14:13:15|Paul Kekai Manansala|Fwd: Ancient map of Africa from China poses questions |

Note that in Hapgood's book mentioned in an earlier post, he considers a very accurate Chinese map as the capstone of his theory.

He believed that the map used a grid system and demonstrated a knowledge of how to accurately determine longitude in the 12th century.

The map mentioned below is not nearly so precise, but is the first known accurate rendering of the African continent.

Regards,  
Paul Kekai Manansala

---

Ancient map of Africa poses questions

Image:

[http://cooltech.iafrica.com/cm\\_pics/technology/12-1845-0-0\\_162059.jpg](http://cooltech.iafrica.com/cm_pics/technology/12-1845-0-0_162059.jpg)

The unveiling in South Africa's parliament on Monday of a replica of an ancient Chinese map of the then known world which includes a recognisable outline of Africa is raising intriguing questions of which foreigners first explored the continent.

"The idea is to take us beyond what we have been ... brainwashed into believing" declared Speaker Frene Ginwala at the opening of the exhibition, which includes other maps and rock art.

The "Da Ming Hun Yi Tu", the Amalgamated Map of the Great Ming Empire, dates back to 1389, decades before the first European voyages to Africa.

Among recognisable features are the Nile River and South Africa's Drakensberg mountain range.

The map also shows a great lake, covering almost half the continent's land mass. Researchers suggest it may have been drawn on the basis of an Arab legend that stated "farther south from the Sahara Desert is a great lake, far greater than the Caspian Sea".

(The biggest lake in Africa, Lake Victoria, is in fact only a fifth of the size of the Caspian Sea.)

"We have the worlds best researchers working on it," said parliaments senior researcher Heindri Bailey, who was hesitant about drawing conclusions from it.

"Until we are able to gain the knowledge we wont speculate on it."

The original of the map is housed in Beijing where it has remained wrapped up, sealed and stowed behind a locked door since the fall of Chinas last emperor in 1924. Fewer than 20 people have had access to it since then.

The digitised reproduction of the map on silk is almost four metres (around 12 feet) high and more than four metres across.

Place names are written mostly in Manchu, a now virtually extinct language, and still in need to be translated.

Karen Harris of the historical and heritage studies department at the University of Pretoria said that as early as the 1st century AD records had been found in China mentioning places in Africa.

"They had the capability, definitely," she said. "Theres not so much evidence to prove it, but it isnt a closed book yet."

A picture dated 11 November 2002 shows a detail on the Da Ming Hun Yi Tu (the Amalgamated Map of the Great Ming Empire) dating back to 1389 which is arguably the oldest world map in existence that accurately reflects the African continent.

Harris said that at the time the Chinese were seeking tribute and not trade for the emperor and therefore would not have set up bases or left behind significant markings as was the case with Europeans.

This, she said, would make it difficult to uncover evidence in support of Chinese having been there, adding: "You wouldnt find human remains because the Chinese took their bodies back to their ancestral lands."

But Bailey said some circumstantial evidence existed in South Africa to suggest the Chinese had navigated around Africa long before Bartolomeu Dias rounded the Cape of Good Hope in 1488.

"Chinese pottery has been found in (South Africas northern) Limpopo Province dating back to around the 13th century and there's rock art in the Eastern Cape depicting Chinese-looking characters," Bailey

said.

British amateur researcher Gavin Menzies, a submarine engineer, argues in "1421", a book which came out this month, that Chinese admiral Zheng He circumnavigated the globe between 1421 and 1423, 100 years before the crew of Portuguese explorer Ferdinand Magellan, who was killed en route.

Zheng He, a eunuch who never travelled with fewer than 300 ships, the biggest carrying 1000 people, is long known to have visited Asia, India, Gulf countries, and Somalia, from where he took back giraffes and lions.

The official history also mentions "Franca" (France and Portugal) and Holland, with the Hollanders described as tall people with red hair and beards.

To meet them in their homeland, Zheng He would have had to sail round the southern tip of Africa.

This is the first time that a copy of the map has been shown outside China. The original is a derivative of an even earlier one dated 1320, which was believed to have been destroyed.

That was before Zheng He's birth (he lived from 1371 to 1435), which deepens the mystery.

Some of the later European maps on show in parliament illustrate dragons, snakes and one-eyed monsters in the inland regions.

AFP

| 217|2002-11-26 23:47:39|LV Hayes|Re: Origin of Tai Voiceless Aspirates|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>

>Date: Thu, 21 Nov 2002 13:54:37 -0000

>

>I dug this up in an article \_cached\_ by Google from [www.reseau-](http://www.reseau-asie.com)

>asie.com (a search on Kra-dai and Ostapirat will get it from

>[www.google.co.uk](http://www.google.co.uk) ; the URL now accesses something different!):

Thanks for the tip!

>Cette hypothèse est aujourd'hui toujours d'actualité par

>Ostapirat (2001) et critiquée entre autres, par Thurgood

>(1994).'

I haven't seen anything by Ostapirat, though I understand that he has been publishing articles on SEAsian languages for a decade or more.

I have read Thurgood's article, and while he makes some good points, I'm not entirely satisfied. Too often, modern-day critics look for one weak point, then declare a finding or hypothesis or theory dead, when all it is is one (or several) weak points, which signify no needed change to the basic proposition.

>The reference is 'Ostapirat W. (2001), 'Kra-dai and  
>Austronesian : notes on some phonological correspondences'.

Yes, that is apparently the article in which Ostapirat shows some probative evidence for the AN + Kradai connection, and it is apparently based on his PhD dissertation (date unknown).

>Is the following perchance the catalogue entry for his thesis?

>

>Reference Type:Edited Book

>Author:Ostapirat, Weera

>Year:1999

>Title:Proto-Kra

>University:Berkeley

>Copies:1

>Location:L

>Call number:xx

>

>(from <http://www.linguistics.berkeley.edu/Hemloc/HEMLOC.html> )

I have no idea. I haven't gotten around to trying to track down a copy of the dissertation. I tried first to e-mail Ostapirat, but either the address is bad or he chose not to reply.

>In Tai, the first type of change occurred after proto-Tai,  
>and only yielded aspirates in some dialects. Strangely  
>enough, the only cognate of a Tai voiced plosive I can see  
>in the Mak sample looks like a reflex of proto-Kradai \*?g,  
>which seems to have deglottalised to proto-Tai \*g, leaving  
>the series of preglottalised stops incomplete. Maybe I  
>haven't looked hard enough. The proto-Tai affricates aren't  
>well represented either; \*dZ is the only one I can see a  
>correspondence for.

Who are your sources for Proto-Kradai and Proto-Tai reconstructions?

>Have you ever looked and failed with the second type?

Well, if you're asking if every example of an aspirate can be shown to reflect \*CVh- > \*Ch-, then the answer must be "no". I think this can be shown in a large number of cases, but I really have no statistics on file to back up that declaration.

>The frequent change proto-Tai \*tr- > Central Tai dialect  
>th- shows restructuring, and the various words for 'who?'  
>(example in Li) - Siamese 'khrai' from 'khon rai' and,  
>elsewhere, 'phrai' from 'phu: rai' or its reflexes, show  
>that syllables can coalesce. I know very few Austro-  
>Asiatic or Austronesian words.

See the Austric glossary on my web page (address below) and you will find a good number of AA words and AN proto-forms.

I wasn't aware of the cited Tai coalescence, but this is generally what I believe has happened in all branches of AA from an early date.

>The only hints of vanished prefixes (as opposed to the  
>abundant classificatory prefixes) that I am aware of are:  
>  
>(1) the voicing alternation that Li discusses at the start  
>of his section on tones. I wouldn't build an Austro-Welsh  
>family on that basis! I suppose the tone alternations

Looks like you left a thought unfinished.

>(2) the apparently meaningless prefixes k(r)a- and ta- (in  
>Siamese).

At first glance, I would suggest that these prefixes are borrowed from Mon-Khmer. The term "presyllable" is usually preferred to describe such prefix-like constructions when no prefixal function can be identified.

The prefix kra occurs only in Khmer, as far as I know; however, if \*kra as in Proto-Kradai is reconstructible back to that early level, then it may be Khmer that did the borrowing.

>Should I take it that the purely Tai-Kadai aspects of this  
>discussion are off-topic?

That's not for me to say, but I would recommend that Tai-Kadai not be excluded from discussion in a forum entitled Austric.

Unfortunately, I'm not able to discuss things with more ardor. I caught the flu yesterday, so am doing well to write this much.

LV Hayes

<http://home.att.net/~lvhayes/home.htm>

| 218|2002-11-27 05:12:56|jojomalig|Re: Fwd: Ancient map of Africa from China poses questions|

As for ancient maps, the Chinese created a lot of good ones. At the K Center in Pampanga are ancient Chinese maps of Luzon and the Philippines, made centuries before European contact.

regards,  
Jojo Malig

--- In austric@y..., "Paul Kekai Manansala" wrote:

> Note that in Hapgood's book mentioned in an earlier post, he  
> considers a very accurate Chinese map as the capstone of his theory.

>

> He believed that the map used a grid system and demonstrated a  
> knowledge of how to accurately determine longitude in the 12th  
> century.

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> The map mentioned below is not nearly so precise, but is the first  
> known accurate rendering of the African continent.

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> Regards,  
> Paul Kekai Manansala

>

> ---

> Ancient map of Africa poses questions

>

> Image:

> [http://cooltech.iafrica.com/cm\\_pics/technology/12-1845-0-0\\_162059.jpg](http://cooltech.iafrica.com/cm_pics/technology/12-1845-0-0_162059.jpg)

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> an ancient Chinese map of the then known world which includes a  
> recognisable outline of Africa is raising intriguing questions of  
> which foreigners first explored the continent.

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> Among recognisable features are the Nile River and South Africas  
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> The map also shows a great lake, covering almost half the  
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> (The biggest lake in Africa, Lake Victoria, is in fact only a fifth  
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> The original of the map is housed in Beijing where it has remained  
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> Place names are written mostly in Manchu, a now virtually extinct  
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> the University of Pretoria said that as early as the 1st century AD  
> records had been found in China mentioning places in Africa.  
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> "They had the capability, definitely," she said. "There's not so much  
> evidence to prove it, but it isn't a closed book yet."  
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> accurately reflects the African continent.  
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> not trade for the emperor and therefore would not have set up bases  
> or left behind significant markings as was the case with Europeans.  
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> This, she said, would make it difficult to uncover evidence in  
> support of Chinese having been there, adding: "You wouldn't find  
> human remains because the Chinese took their bodies back to their  
> ancestral lands."  
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> to suggest the Chinese had navigated around Africa long before  
> Bartolomeu Dias rounded the Cape of Good Hope in 1488.  
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> Zheng He, a eunuch who never travelled with fewer than 300 ships,  
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> The official history also mentions "Franca" (France and Portugal)  
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> 1320, which was believed to have been destroyed.  
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> Some of the later European maps on show in parliament illustrate  
> dragons, snakes and one-eyed monsters in the inland regions.  
>  
>  
> AFP  
| 219|2002-11-27 05:15:05|jojomalig|Re: New file uploaded to austric|  
--- In austric@y..., a.manansala@a... wrote:  
> Jojo,  
>  
> These look like theories pertaining possibly only to the

Austronesian  
> migrations.

You are correct on that, Paul. It is merely to compare the  
Austronesian migration theories.

regards,

Jojo

| 220|2002-11-27 05:47:59|Richard Wordingham|Re: Origin of Tai Voiceless Aspirates|  
--- In austric@y..., LV Hayes wrote:  
> Who are your sources for Proto-Kradai and Proto-Tai reconstructions?

For Proto-Tai, I use Li's forms.

My Proto-Kradai reconstructions are a leap of faith, tweaking Proto-Tai in the light of Li's comments on the non-Tai Tai-Kradai languages (mostly Sui) and the small sample of Mak I have. It's not the best of sources, but it's all I've got. I'd be very nervous of reconstructing a Proto-Kradai vowel, or the precursor of a Proto-Tai preaspirated nasal.

However, Li's Proto-Tai seems to have been checked against the non-

Tai languages, so it's not a bad starting point. I had wondered why he so often included words evidenced from only one of the Tai groups. However, I have now noticed that several of the Mak words have Tai cognates only in Northern Tai, so I wonder if he included words with limited distribution because he knew of cognates outside Tai.

> > Have you ever looked and failed with the second type?

>

> Well, if you're asking if every example of an aspirate can be shown

to

> reflect \*CVh- > \*Ch-, then the answer must be "no". I think this

can be

> shown in a large number of cases, but I really have no statistics

on file

> to back up that declaration.

I was actually wondering if you'd compared Li's reconstructions with AA / PAN. One wouldn't expect many matches.

> > I know very few Austro-Asiatic or Austronesian words.

> See the Austric glossary on my web page (address below) and you

will find a

> good number of AA words and AN proto-forms.

So I should do the work myself. It looks like an on-line job. At least I can guess the Siamese form for a lot of the Austro-Tai reconstructions you quote.

> > I suppose the tone alternations

> Looks like you left a thought unfinished.

... could correspond to vanished suffixes.

> > (2) the apparently meaningless prefixes k(r)a- and ta- (in

> > Siamese).

>

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from

> Mon-Khmer. The term "presyllable" is usually preferred to describe

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> prefix-like constructions when no prefixal function can be

identified.

> The prefix kra occurs only in Khmer, as far as I know; however, if

\*kra as

> in Proto-Kradai is reconstructible back to that early level, then

it may be

> Khmer that did the borrowing.

The reflexes of PAA \*nqlaŋ, namely Brao k&dMMN, Rawe kr&d&&N, Sapuan kr&d1axN 'bone' look very like Siamese kraduuk 'bone', but I think that's just outrageous coincidence. (Mak lacks the presyllable.) I'm not sure what the Tai cognate is supposed to be; Siamese kro:ng 'form, skeleton' looks rather forced, but not impossible.

I get the distinct impression that Li wouldn't have been happy reconstructing such prefixes at the Proto-Tai level. He generally ignores presyllables. I'll check and get back if it was only the clearly classificatory prefixes that he's ignored.

> Unfortunately, I'm not able to discuss things with more ardor. I

caught

> the flu yesterday, so am doing well to write this much.

I appreciate the reply and wish you a speedy recovery.

Richard.

| 221|2002-11-27 11:20:31|a.manansala@attbi.com|Re: New file uploaded to austric|  
Thanks Jojo, on second look it seems that most of the upper limits for migrations are not given.

The dates look mostly like those for secondary migrations as they are a little late. For example, the Lapita culture, which is often considered either Proto-Oceanic or Proto-Polynesian, is usually dated to at least 1,600 BCE.

Given a Philippine or East Indonesian launching pad for Proto-Lapita all the given dates on the map are too late.

Regards,  
Paul Kekai Manansala

>  
>  
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>>  
>> These look like theories pertaining possibly only to the  
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> You are correct on that, Paul. It is merely to compare the  
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>  
| 222|2002-11-27 16:22:00|TTT UUU|Re: Fwd: Ancient map of Africa from China poses  
questions|

Paul

was your posting about the Chinese silk map from a South African newspaper? Newspapers writers have to fill up space and often don't look into the subject so I took it with a grain of salt. There is little evidence to suggest that these African maps in China originated with Chinese explorers. The silk African map is not dated?

Let's look at the example of Madagascar, colonized in about 1000 AD by family groups (with women) with rice seed and perhaps other agrarian technology. They may/probably have known exactly where they were going. Did Indonesian (Austric)/ Nunsanto people discover Madagascar at the time of the great dispersal 7,500 BC (Oppenheimer) and map the African coast or did they pick up this information from Arab/Egyptian traders closer to the 1000 AD colonization date? I am trying to get more information via a contact (Firenina Merina) at the lemurdolls.com Madagascar site I posted earlier.

The Ptolomy-Alexandrian map source (Greek Egyptian dynasty, perhaps 300 BC) is older than the Chinese silk map and shows a vastly more accurate African outline. We don't know where the Alexandrians derived their original charts but some of Hapgood's analyses point back to the end of the last ice age, the time of the great flooding of SE Asia (detailed by Oppenheimer).

I appreciate that China Post newspaper info about the stone walls found 25 meters below sea level near Taiwan (and Yonaguni Is.). These and other flooded villages/towns/cities (?) are the remains of unnamed civilization(s) much older than the Chinese Shang Dynasty (founded approx. 1400 BC) expansion from the Yellow river near Anyang. Sub-marine archaeology on the flooded coastal shelf in SE Asia seems to be the hot area to explore in the next 100 years, costly but needed.

Robin Day, S. Korea

regards,  
Jojo Malig

--- In austric@y..., "Paul Kekai Manansala" wrote:  
> Note that in Hapgood's book mentioned in an earlier post, he  
> considers a very accurate Chinese map as the capstone of his theory.  
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> He believed that the map used a grid system and demonstrated a  
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> language, and still in need to be translated.

>

> Karen Harris of the historical and heritage studies department at

> the University of Pretoria said that as early as the 1st century AD

> records had been found in China mentioning places in Africa.

>

> "They had the capability, definitely," she said. "Theres not so

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> evidence to prove it, but it isn't a closed book yet."

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>

> A picture dated 11 November 2002 shows a detail on the Da Ming Hun  
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> But Bailey said some circumstantial evidence existed in South  
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 > and Holland, with the Hollanders described as tall people with red  
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> To meet them in their homeland, Zheng He would have had to sail  
 > round the southern tip of Africa.  
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- > This is the first time that a copy of the map has been shown outside
- > China. The original is a derivative of an even earlier one dated
- > 1320, which was believed to have been destroyed.
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- > That was before Zheng He's birth (he lived from 1371 to 1435), which
- > deepens the mystery.
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- > Some of the later European maps on show in parliament illustrate
- > dragons, snakes and one-eyed monsters in the inland regions.
- >
- >
- > AFP

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| 223|2002-11-27 16:22:09|TTT UUU|Re: Fwd: Ancient map of Africa from China poses questions|

Paul

was your posting about the Chinese silk map from a South African newspaper? Newspapers writers have to fill up space and often don't look into the subject so I took it with a grain of salt. There is little evidence to suggest that these African maps in China originated with Chinese explorers. The silk African map is not dated?

Let's look at the example of Madagascar, colonized in about 1000 AD by family groups (with women) with rice seed and perhaps other agrarian technology. They may/probably have known exactly where they were going. Did Indonesian (Austric)/Nunsanto people discover Madagascar at the time of the great dispersal 7,500 BC (Oppenheimer) and map the African coast or did they pick up this information from Arab/Egyptian traders closer to the 1000 AD colonization date? I am trying to get more information via a contact (Firenina Merina) at the lemurdolls.com Madagascar site I posted earlier.



The Ptolomy-Alexandrian map source (Greek Egyptian dynasty, perhaps 300 BC) is older than the Chinese silk map and shows a vastly more accurate African outline. We don't know where the Alexandrians derived their original charts but some of Hapgood's analyses point back to the end of the last ice age, the time of the great flooding of SE Asia (detailed by Oppenheimer).

I appreciate that China Post newspaper info about the stone walls found 25 meters below sea level near Taiwan (and Yonaguni Is.). These and other flooded villages/towns/cities (?) are the remains of unnamed civilization(s) much older than the Chinese Shang Dynasty (founded approx. 1400 BC) expansion from the Yellow river near Anyang. Sub-marine archaeology on the flooded coastal shelf in SE Asia seems to be the hot area to explore in the next 100 years, costly but needed.

Robin Day, S. Korea

regards,  
Jojo Malig

--- In austric@y..., "Paul Kekai Manansala" wrote:  
> Note that in Hapgood's book mentioned in an earlier post, he  
> considers a very accurate Chinese map as the capstone of his theory.  
>  
> He believed that the map used a grid system and demonstrated a  
> knowledge of how to accurately determine longitude in the 12th  
> century.  
>  
> The map mentioned below is not nearly so precise, but is the first  
> known accurate rendering of the African continent.  
>  
> Regards,  
> Paul Kekai Manansala  
>  
> ---  
> Ancient map of Africa poses questions  
>  
> Image:  
> [http://cooltech.iafrica.com/cm\\_pics/technology/12-1845-0-0\\_162059.jpg](http://cooltech.iafrica.com/cm_pics/technology/12-1845-0-0_162059.jpg)  
>  
> The unveiling in South Africa's parliament on Monday of a replica  
> of  
> an ancient Chinese map of the then known world which includes a  
> recognisable outline of Africa is raising intriguing questions of  
> which foreigners first explored the continent.  
>  
> "The idea is to take us beyond what we have been ... brainwashed

> into believing" declared Speaker Frene Ginwala at the opening of the

> exhibition, which includes other maps and rock art.

>

> The "Da Ming Hun Yi Tu", the Amalgamated Map of the Great Ming

> Empire, dates back to 1389, decades before the first European

> voyages to Africa.

>

> Among recognisable features are the Nile River and South Africas

> Drakensberg mountain range.

>

> The map also shows a great lake, covering almost half the

continents

> land mass. Researchers suggest it may have been drawn on the basis

> of an Arab legend that stated "farther south from the Sahara Desert

> is a great lake, far greater than the Caspian Sea".

>

> (The biggest lake in Africa, Lake Victoria, is in fact only a fifth

> of the size of the Caspian Sea.)

>

> "We have the worlds best researchers working on it," said

> parliaments senior researcher Heindri Bailey, who was hesitant

about

> drawing conclusions from it.

>

> "Until we are able to gain the knowledge we wont speculate on it."

>

> The original of the map is housed in Beijing where it has remained

> wrapped up, sealed and stowed behind a locked door since the fall

of

> Chinas last emperor in 1924. Fewer than 20 people have had access

to

> it since then.

>

> The digitised reproduction of the map on silk is almost four metres

> (around 12 feet) high and more than four metres across.

>

> Place names are written mostly in Manchu, a now virtually extinct

> language, and still in need to be translated.

>

> Karen Harris of the historical and heritage studies department at

> the University of Pretoria said that as early as the 1st century AD

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| 224|2002-11-27 18:24:07|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

- >
- > Paul

> The Ptolomy-Alexandrian map source (Greek Egyptian dynasty, perhaps 300 BC) is > older than the Chinese silk map and shows a vastly more accurate African outline

No, Ptolemy did not leave us any maps and he did not have a good idea of the shape of the continent based on his tables of latitudes and longitudes. His knowledge seemed to stop around present-day Tanzania.

Maps derived from his Geographia showed that he even created an error by changing the idea that the continent was surrounded by ocean. Instead, he depicted the Indian Ocean as a massive land-locked lake with Africa extending far to the south and connecting with Asia.

Regards,

Paul Kekai Manansala

| 225|2002-11-28 00:47:37|TTT UUU|Re: Fwd: Ancient map of Africa poses questions|

I was referring to the Egyptian-Greek Ptolemaic Dynastic period founded by one of Alexander's generals (the Greek Pharos). Paul, I believe you refer individually to Claudius Ptolemy (85-165AD) the Greek astronomer or his group of geographers associated with the Library of Alexandria at the time. They did produce maps (originals do not survive) and Hapgood refers to the Cantino Planisphere map (and other surviving Mediterranean maps) as examples/copies that derive their accuracy/origin from the group at the Library of Alexandria. This was one of Hapgood's main theses. Did Claudius Ptolemy originate the Alexandrian mapping methods or receive the math and maps from earlier peoples? We don't know. I can reply in more detail tomorrow when I have Hapgood's book in front of me. I just read it in September.

Robin Day, S.Korea

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Regards,  
Paul Kekai Manansala

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| 226|2002-11-28 05:03:32|LV Hayes|Re: Origin of Tai Voiceless Aspirates|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>

>Date: Wed, 27 Nov 2002 13:47:43 -0000

>

>For Proto-Tai, I use Li's forms.

The only other person I know of who has worked on Proto-Tai is Andre-George Haudricourt, cf. his "Les phonemes et le vocabulaire du thai commun" in *\_Journal Asiatique\_*, 1948, 236:197-238 (among others), which I have not seen. Benedict, of course, proposes some Tai proto-forms of his own in his various publications.

>My Proto-Kradai reconstructions are a leap of faith,  
>tweaking Proto-Tai in the light of Li's comments on the  
>non-Tai Tai-Kradai languages (mostly Sui) and the small  
>sample of Mak I have. It's not the best of sources, but  
>it's all I've got. I'd be very nervous of reconstructing  
>a Proto-Kradai vowel, or the precursor of a Proto-Tai  
>preaspirated nasal.

I understand, but few others will. In the "real" linguistic world, "leaps of faith" are tantamount to practice of witchcraft.

The AA evidence suggests that preaspirated nasals, i.e. hm-, etc., reflect phonetic conditioning, such that \*Cm- > hm-, etc. Most likely, the \*C involved were continuants.

>I was actually wondering if you'd compared Li's  
>reconstructions with AA / PAN. One wouldn't expect  
>many matches.

No, I wouldn't say that I had compared Proto-Tai reconstructions with AA and/or AN. On the other hand, I have identified potential Proto-Tai cognates to many of my Austric comparisons and included them in the comparative files. Few, if any, of these cognates appear in the online Austric glossary; after all, Proto-Tai is external to the Austric comparison as I see it. I had hoped to get back sometime to this material and do an Austric/Proto-Tai comparison or the like. The chances of this ever occurring now seem ever more remote.

>>See the Austric glossary on my web page (address below)  
>>and you will find a good number of AA words and AN  
>>proto-forms.  
>  
>So I should do the work myself. It looks like an on-line job.

Unless you're a tenured professor somewhere with your own personal flock of unbound apprentices (read PhD candidates), you probably will have to do all the work yourself. Not to worry; it's good for you.

>At least I can guess the Siamese form for a lot of the  
>Austro-Tai reconstructions you quote.

And then find them in a Siamese dictionary, I hope. Using "guessed" forms outright is a no-no, and it appears that Benedict was guilty in a few cases of using such, for no one else can find some of his lexical examples in any known language or dialect, whence the eventual rejection of his work after a period of jubilant approbation. Or so I am told.

>>I suppose the tone alternations could correspond to  
>>vanished suffixes.

If my morphological model for AA (and probably also Austric) is correct, then such suffixes did once occur in the area, if not precisely in Kradai or Proto-Tai.

If one takes Haudricourt's tonogenesis theory seriously, then one must reconstruct a series of glottalized final continuants for an early stage of Viet-Muong, i.e. \*/?m, ?n, ?n~, ?ng, ?l, ?r, ?w, ?y/, where \*/?/ is a glottal stop or feature. There is elsewhere in MK no evidence for such

phonemes, except as secondary developments in registrogenesis scenarios, and this does not appear to be applicable to the VM situation. To explain such a series, one might be able to posit \*/C<sub>aq</sub> > Ca? > C? > ?C/, where \*/(a)q/ is an ancient suffix. Proving that it did happen that way will be much more difficult.

>The reflexes of PAA \*nqlaN, namely Brao k&dMMN, Rawe  
>kr&d&&N, Sapuan kr&d1axN 'bone' look very like Siamese  
>kraduuk 'bone', but I think that's just outrageous  
>coincidence. (Mak lacks the presyllable.)

Benedict (1975:238) tried to compare Thai \*/duuk ~ \*ka/?duuk (Li cites Siamese kra-duuk 'bone', with duuk a reflex of \*/dl[uo]k or \*/dr[uo]k) with AN \*[t]ulang 'bone'. This was Otto Dempwolff's (1938) reconstruction; since then, the AN proto-form has undergone several metamorphoses and its latest version is Robert Blust's \*CugelaN and (the doublet) \*tugelang 'bone', where C denotes a sort of retroflex affricate and N a retroflex n (if I understand the notation correctly); q is of course the post-velar k. Even from the first, the affiliation of \*(ka)?duuk was dubious, though Benedict's inclusion of Kam-Sui \*/dlaak and Proto-Kam-Sui \*tla:k7 'bone' offered a plausible bridge between the forms.

My latest Proto-AA form (see web site) is \*c(i,a,u)(n)qang 'bone', which may be a less than clear correspondent to the AN forms. The idea is that \*qang is the 'bone' root; the rest is affixation, i.e. \*qang > \*qelang, etc. in AN.

In this view, Tai \*/dl/r[uo]k cannot fit at all and thus must be excluded from the Austric 'bone' comparison. Note, however, that Proto-Tai \*kang '(fish) bone' is waiting in the wings, and it is a very plausible correlate via \*qang > \*kang, a regular change per Benedict 1975. Note that he did not cite \*kang in his 1975 glossary of roots.

Thus, while one Benedictian comparison falls on its face, a replacement comparison is easily discoverable. I think you will find this to occur on numerous occasions, so have no fear that if you don't soon find a comparative candidate, none exists. One must search and research the lexicons and examine and re-examine the data before one espies that bit of gold glittering in the mud.

To get back to the correlation of Brao, Rawe, and Sapuan d to Siamese d, coincidence is the best answer. Note that the d of the MK forms corresponds to t in Paul Sidwell's Proto-South Bahnaric \*n@ting 'bone'. Since Brao, Rawe, and Sapuan are from a different branch of Bahnaric, one may assume that all these 'bone' forms derive from a single ancient



Bahnaric form, whose exact phonological structure we can only guess at. It does not appear to come from Chamic \*tulaang 'bone'.

>I'm not sure what the Tai cognate is supposed to be;  
>Siamese kro:ng 'form, skeleton' looks rather forced, but  
>not impossible.

Siamese kro:ng looks to me like it could be a borrowing from MK, but I have no cognate ready at hand.

>I get the distinct impression that Li wouldn't have been  
>happy reconstructing such prefixes at the Proto-Tai level.  
>He generally ignores presyllables. I'll check and get  
>back if it was only the clearly classificatory prefixes  
>that he's ignored.

Ignorance of presyllables evidently reflects theoretical thinking of the 1950s and 60s (and a long time thereafter). Gerard Diffloth also generally ignored presyllables in his treatment of Wa and Proto-Waic. Granted that the presyllables are a pain to work with, but I think they provide crucial evidence to grasping insight to the real structure of ancient areal languages, which is reflected in my morphological model (cf. Hayes 1992).

>I appreciate the reply and wish you a speedy recovery.

Thanks! I'm sure it's just a matter of time.

LV Hayes  
| 227|2002-11-28 16:36:16|TTT UUU|Re: Fwd: Ancient map of Africa from China poses questions|

I have Hapgood's book in front of me this morning. Oops! I should have said De Canerio (map) instead of "Cantino" in my last post. Page 117, etc. states Hapgood's ideas of the very ancient source of the very accurate map of Africa. The Portolan (Port to Land) maps he shows are extremely accurate and he concluded they were **copies** from ancient sources. They could not be created in Europe at the time as the math did not exist. His book is well worth reading in detail. [He was mistaken about the age of the Cucillo pyramids, Mexico. These have been dated more recently,]

Hapgood coins a name for the unknown map source in the title of his book Maps of the Ancient Sea Kings. Who were the Ancient Sea Kings? Some other writers call this the Elder Culture. Were they Austric, pre-dynastic Egyptian, related to the sunken walls and cities of Taiwan-Okinawa and India (off Guderat)? We need more submarine archaeology on the continental shelves.

I don't know if Hapgood is still alive. His book was published in 1966. Will do some digging.

Robin Day S. Korea

**TTT UUU** wrote:

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Regards,  
Paul Kekai Manansala

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| 228|2002-11-29 01:24:56|Richard Wordingham|Mon Conch|  
While looking at the Austric gloss for 'crustacean' in the Austric website at <http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm> , I saw Old Mon saN 'conch' given as a derivative of Proto-Austr(o-Asiat)ic \*zaN. ('N' = velar nasal.) Is there not a strong possibility that the Mon word actually derives from Sanskrit s'aNkha 'conch'? (s' is the palatal sibilant, written 'z' by the Harvard school.) FWIW, Siamese has this word (saN) and spells it as though it were borrowed from Sanskrit via Pali (so ~~,not, in Siamese~~).

~~The Sanskrit word 's'aNkha' may well be of Dravidian origin; see for example the discussions at~~

~~<http://groups.yahoo.com/group/cybalist/message/9436> and <http://groups.yahoo.com/group/cybalist/message/9494>.~~

~~Richard:~~

| 229|2002-11-29 02:00:42|Richard Wordingham|Re: Origin of Tai Voiceless Aspirates|

--- In austrie@y..., LV Hayes wrote:

> >From: "Richard Wordingham"

> >Date: Wed, 27 Nov 2002 13:47:43 -0000

> >For Proto-Tai, I use Li's forms.

>

> The only other person I know of who has worked on Proto-Tai

> is Andre-George

> Haudricourt, cf. his "Les phonemes et le vocabulaire du thai

> commun" in

> \_Journal Asiatique\_, 1948, 236:197-238 (among others), which

> I have not

> seen. Benedict, of course, proposes some Tai proto-forms of

> his own in his

> various publications.

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> >My Proto-Kradai reconstructions are a leap of faith,

> >tweaking Proto-Tai in the light of Li's comments on the

> >non-Tai Tai-Kradai languages (mostly Sui) and the small

> >sample of Mak I have. It's not the best of sources, but

> >it's all I've got. I'd be very nervous of reconstructing

> >a Proto-Kradai vowel, or the precursor of a Proto-Tai

> >preaspirated nasal.

>

> I understand, but few others will. In the "real" linguistic

> world, "leaps

> of faith" are tantamount to practice of witchcraft.

I'd say 'favoured daughter' approach plus lack of contrary evidence.

Mind you, I must be missing something in my Mak sample. I can't see any cognates for proto-Tai words starting \*b or \*d!

BTW, how do you categorise Li's reconstruction of Proto-Tai \*dl and \*dr from patterns of apparent reflexes of \*r- and \*l-? Divine inspiration?

> The AA evidence suggests that preaspirated nasals, i.e. hm-,

> etc., reflect

> phonetic conditioning, such that \*Cm- > hm-, etc. Most likely, the

\*C

> involved were continuants.

I was just thinking of the immediate issue that Li raises, namely that sometimes the Sui cognates start with a preaspirated nasal and sometimes with a preglottalised nasal. Li gives examples of the contrast (not minimal pairs) when justifying the reconstruction of Proto-Tai \*ʔb.

- > > At least I can guess the Siamese form for a lot of the
- > > Austro-Tai reconstructions you quote.
- >
- > And then find them in a Siamese dictionary, I hope.

My Thai's bad enough that I check almost everything. I know very few words that aren't in the Thai-English dictionaries, and Li gives most of those that look ancient.

- > > > I suppose the tone alternations could correspond to
- > > > vanished suffixes.
- >
- > If my morphological model for AA (and probably also Austric)
- > is correct,
- > then such suffixes did once occur in the area, if not
- > precisely in Kra-dai
- > or Proto-Tai.
- >
- > If one takes Haudricourt's tonogenesis theory seriously, then one

must

- > reconstruct a series of glottalized final continuants for an
- > early stage of
- > Viet-Muong, i.e. \*/ʔm, ʔn, ʔn̥, ʔng, ʔl, ʔr, ʔw, ʔy/, where \*/ʔ/ is

a

- > glottal stop or feature. There is elsewhere in MK no
- > evidence for such
- > phonemes, except as secondary developments in registrogenesis
- > scenarios,
- > and this does not appear to be applicable to the VM
- > situation.

Are the examples in Gage (1985, Glottal Stops and Vietnamese Tonogenesis, in 'For Gordon H. Fairbanks', ed. V.Z. Acson & R.L. Leed, Oceanic Linguistics Special Publication No. 20, University of Hawaii Press, pp21-36) that Thurgood cites in <http://www.csuchico.edu/~gt18/Papers/Vietnamese&tonogenesis.pdf> in this category?

- > To explain
- > such a series, one might be able to posit \*/Caq > Ca? > C? >
- > ?C/, where
- > \*/(a)q/ is an ancient suffix. Proving that it did happen
- > that way will be
- > much more difficult.

Do we need /?m/ as opposed to /m?/? Li writes that the final glottal stop that is a feature of the low falling (21) tone of the Lungchow dialect follows rather than precedes the otherwise final nasal.

- > Even from the first, the affiliation of \*(ka)?duuk was dubious,

though

- > Benedict's inclusion of Kam-Sui \*/?dlaak and Proto-Kam-Sui
- > \*tla:k7 'bone'
- > offered a plausible bridge between the forms.

Do you actually mean tl > ?dl for Kam-Sui with a parallel change for Tai? (The best evidence for \*/?dl that Li found in Tai was in Saek, which Ethnologue (why?) excludes from Tai.) If this is what happened, I can have no confidence in my witchcraft!

- > My latest Proto-AA form (see web site) is \*c(i,a,u)(n)qang
- > 'bone', which
- > may be a less than clear correspondent to the AN forms. The
- > idea is that
- > \*qang is the 'bone' root; the rest is affixation, i.e. \*qang
- > > \*qelang,
- > etc. in AN.

I'm not sure I understand your notation. I take it round brackets indicate a multiple choice or, given only one option, an optional part, but what do square brackets mean?

- > In this view, Tai \*/?dl/r[uo]k cannot fit at all and thus must
- > be excluded
- > from the Austric 'bone' comparison. Note, however, that
- > Proto-Tai \*kang
- > '(fish) bone' is waiting in the wings, and it is a very plausible
- > correlate via \*qang > \*kang, a regular change per Benedict
- > 1975. Note that
- > he did not cite \*kang in his 1975 glossary of roots.
- >

- > Thus, while one Benedictian comparison falls on its face, a
- > replacement
- > comparison is easily discoverable. I think you will find
- > this to occur on
- > numerous occasions, so have no fear that if you don't soon find a
- > comparative candidate, none exists. One must search and research

the

- > lexicons and examine and re-examine the data before one
- > espies that bit of
- > gold glittering in the mud.

And beware the iron pyrites!

- > Note that the d of the MK forms
- > corresponds to t in Paul Sidwell's Proto-South Bahnaric
- > \*n@ting 'bone'.

The archives have mangled n @ t i n g; on cybalist we use '&' instead of '@' so as to avoid this problem.

- >>I'm not sure what the Tai cognate is supposed to be;
- >>Siamese kro:ng 'form, skeleton' looks rather forced, but
- >>not impossible.
- >
- > Siamese kro:ng looks to me like it could be a borrowing from
- > MK, but I have
- > no cognate ready at hand.

Fair point. The vowel's wrong for an inherited word, though Siamese vowel lengths are unreliable. I apologise for misspelling the word; it's 'khro:ng', not 'kro:ng'.

- >>I get the distinct impression that Li wouldn't have been
- >>happy reconstructing such prefixes at the Proto-Tai level.
- >>He generally ignores presyllables. I'll check and get
- >>back if it was only the clearly classificatory prefixes
- >>that he's ignored.

From rereading Li's introduction, it seems that he has recorded the fused prefixes, and thought they mattered, but not had not devoted any time to analysing them. He has stripped off the classificatory prefixes that are independent words, such as plaa 'fish' and 'nok'

(etc.) 'bird'. The presyllables seems to be restricted to SW Tai, so Mon Khmer influence looks very plausible.

Richard.

| 230|2002-11-29 08:45:47|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

But the De Canerio map dates only to about 1502 not 1389 like the Chinese map.

It may be that there was an ancient Alexandrian source for these maps but we do not have any extant evidence. The use of Alexandria as a meridian may extend back only to Ptolemy who made his own observations from this city (he was an Upper Egyptian).

Regards,

Paul Kekai Manansala

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| 231|2002-11-29 09:09:22|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

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Before the advents of clocks, this was done astronomically, usually using lunar time, and was very limited in accuracy because of the equipment available.

The earliest map to show accurate longitudes is the 12th century Chinese map mentioned by Hapgood.

That actually makes sense as the Chinese were known to have mechanical clocks before this time.

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| 232|2002-11-29 09:09:40|LV Hayes|Re: Mon Conch|  
>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>  
>Date: Fri, 29 Nov 2002 09:24:50 -0000  
>Subject: [austrie] Mon Conch  
>  
>While looking at the Austrie gloss for 'crustacean' in the  
>Austrie website at [http://home.att.net/~lvhayes/Langling/](http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm)  
>Glossary/glosspg1.htm , I saw Old Mon saN 'conch' given as  
>a derivative of Proto-Austr(o-Asiat)ic \*zaN. ('N' = velar  
>nasal.) Is there not a strong possibility that the Mon  
>word actually derives from Sanskrit s'aNkha 'conch'? (s'  
>is the palatal sibilant, written 'z' by the Harvard school.)

Yes, Sanskrit s'an.kha is a plausible source of Old Mon san. 'conch'.  
However, given the demonstrated PMK shift \*/z > s/, it is equally plausible  
that Old Mon san. reflects retention of PAA \*zang. I have no way of  
telling one way or the other, but study of ancient Mon texts could provide  
eventually a definitive answer.

In any case, whatever the source of the Old Mon form should turn out to be,  
the integrity of the 'crustacean' comparison is not impaired.

LV Hayes

| 233|2002-11-29 20:38:40|TTT UUU|Re: Fwd: Ancient map of Africa from China poses questions|

Paul, the whole thrust of Hapgood's book is about these mysterious peoples he calls Ancient Sea Kings, who left us these copies of incredible maps that show parts of north Europe at the end of the last glaciation, and parts of Antarctica (Queen Maud's Land) exposed from existing ice (pre-ice age). Yes the De Canerio map is clearly dated in the text from 1502 but Hapgood's evidence points to the antiquity of the original data. (All the ancient works survive as copied/ altered manuscripts). These maps vastly predate Ptolemaic Egypt (approx. 100BC-200AD) and the Chinese Sheng dynasty (1,400 BC). The Chinese were not mapping the world prior to and just after the last ice age now were they? You and I and the rest of the world do not know exactly what groups produced these very old maps but Hapgood coined The Sea Kings name and we need to follow this lead.

Hapgood explains the problem of calculating longitude very clearly.

Essentially, historians have to readjust their thinking. As an example agriculture was not originally invented in Iraq and Iran. Author Joseph Campbell suspected it was much older and probably involved tropical root crops in SE Asia and Africa but these roots do not preserve well in the heat. The continent of SE Asia prior to sea floods at 7,500 BC probably had numerous human communities involved in agriculture, sea trading, astronomy and megalithic architecture. We will have to wait for more submarine excavations/ explorations.

We really must follow up on what people made of Hapgood's book after it was released in 1966. Perhaps other academics trashed it with justification. I don't know yet. If we accept Hapgood's main thesis then intelligent human history is very old indeed, a lot of knowledge has been lost and all the history books have to be re-written.

Comments please.

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| 234|2002-11-29 21:00:29|TTT UUU|Re: Fwd: Ancient map of Africa from China poses questions|

Paul, Hapgood lays out his calculations in detail. Sand clocks and water clocks predate mechanical clocks by far and they are quite accurate when calibrated. While at sea clocks are still unreliable in calculating distance travelled over time past as ship are in a changing current and changing wind. **The Vikings** had no clock device. We have no idea what the **Ancient Sea Kings** used to navigate. The **Polynesians** used several methods to navigate but no clock device. I remember a long article in National Geographic about Polynesian navigation. Am getting far from my specialty here so I leave it to others.

*a.manansala@attbi.com* wrote:

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| 235|2002-11-30 01:38:14|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

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- > Paul, Hapgood lays out his calculations in detail. Sand clocks and water clocks
- > predate mechanical clocks by far and they are quite accurate when calibrated.

These types of clocks are less reliable than astronomical methods and are not useful because one has to travel with the clock from place to place to determine longitude.

I'm not doubting the ability to determine longitude at early times, just that there is no evidence it was used in the map-making we are discussing.

When referring to extant evidence, the Chinese maps are the oldest. Everything else is speculation.

The Tahitian Tupaia was able to help Cook's crew chart maps of an area from the Marquesas to Fiji all from memory. Apparently he had no trouble with the Western grid system. But its a stretch to say that Austronesians were responsible for the Chinese maps without more evidence.

All this map-making does appear at about the same time within a few centuries at a period when we know that a lot of information was passing between East and West.

Note also that many of the star charts that starting appearing in Europe at the time (post-1300s) were very similar in their arrangement to Chinese star charts.

Regards,

Paul Kekai Manansala

| 236|2002-11-30 01:52:06|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

I think there's some confusion here. Obviously there is no doubt that the navigational ability existed at an earlier time. Such ability is even documented by Europeans among the Austronesians.

Even in the present day, some of these abilities have survived and have been documented in places like Satawal in Micronesia and the Santa Cruz islands in Melanesia.

However, we still are a bit fuzzy on just how the Austronesians determined longitude.

As far as extant evidence goes, the earliest accurate maps are the ones we have been mentioning.

I think they are all connected with Chinese map-making that was aided by the invention of the mechanical clock (8th century AD).

Also, the exploits of Admiral Zheng Ho including his voyages with a massive fleet to Africa are well-known.

It may that the some other knowledge helped the Chinese in their map-making. Obviously that was one reason that I posted the articles here in the first place.

But that's all highly speculative. Especially when you try to connect these post 12th century maps with Sundaland flooding.

From my knowledge, Austronesian mariners did not rely on maps themselves. They basically memorized their sea charts.

Regards,  
Paul Kekai Manansala

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| 237|2002-11-30 11:16:01|a.manansala@attbi.com|Re: Fwd: Ancient map of Africa from China poses questions|

> It may that the some other knowledge helped the Chinese in their map-making.

> Obviously that was one reason that I posted the articles here in the first

> place.

>

To illustrate this, we can note that the Muslim writings mention in several instances that the people of Zabag and Wakwak in the Malay Archipelago traded on the coast of East Africa. There is even a notice of an attempted invasion of Qanbalu in present-day Tanzania by the Wakwak.

So frequently did these Austronesian mariners visit that they were easily able to communicate with the African peoples that lived there.

This knowledge would have been useful for any Chinese expedition to Africa.

Tupaia probably helped cut down the time it required Europeans to map the Pacific considerably.

However, someone with this type of knowledge would have had a much greater impact in earlier times when long-range navigation was still mostly an unknown art.

And I think it would have been very simple for an Austronesian navigator to quickly understand other types of geographic and astronomical symbolism.

Regards,

Paul Kekai Manansala

| 238|2002-12-01 11:05:36|a.manansala@attbi.com|(no subject)|

[http://news.nationalgeographic.com/news/2002/11/1125\\_021126\\_TVVanuatu.html](http://news.nationalgeographic.com/news/2002/11/1125_021126_TVVanuatu.html)

Reporter's Notebook: S. Pacific Ritual Bungee Jumping

Zoltan Istvan

National Geographic Today

November 26, 2002

Vanuatu's world-famous land diving ritual, the Naghol, seems like a traditional form of bungee jumping. But after watching the first diver leap off a wooden tower reaching upwards of 25 meters (75 feet) and crash into the earth below?and the 300-person crowd erupt in cheers?it's obvious that what's happening on Pentecost Island is something radically different.

In late May I took a small bush plane to Pentecost where the villages of Loltafala were about to hold their annual Naghol. As a solo videojournalist, I'm fascinated with traditional cultures and strange rituals; Pentecost is ideal for both. Every year, males of all ages tie vines to their ankles and make daring leaps?head first?off a wooden tower.

[Image: Vanuatu Islander Bungee Jumper]

On Pentecost Island, divers leap off a wooden tower reaching upwards of 25 meters (75 feet) and crash into the earth below.

Historically, a diver's goal was to bless the soil by skimming the earth with the top of his head; the blessing guarantees fertile soil for a bountiful yam harvest.

But these days the ritual extends far beyond yams?it underlies the foundation of tribal society in Pentecost. When a boy is ready for manhood, he proves his courage by jumping off the tower in view of his elders. Watching from the ground, the boy's mother holds a favorite item from his childhood. After his first jump, the item is thrown away, signifying he is no longer a child.

Of course the islanders of Pentecost dive for many reasons. Some jump for the sheer joy of it; others jump to prove they're committed to their ancient traditions; one young man told me he even jumped just to impress a young girl he was courting. Before dives, it's common for men to settle disputes with family, friends, or wives?just in case they die. While it's rare that anyone gets killed, ruptured spleens from accidental crashes are a serious concern. On an island without a hospital, it's an extremely serious injury.

The day before the ceremony began I visited the jump site and climbed the tower. Renee Tari, a local, explained some of the strange taboos that surround the Naghol.

### Lucky Charms

"A diver must not have sex with a woman the day before he jumps?otherwise the jump will go badly and he will be hurt. Also, you must take off any lucky charms," said Tari. "A diver died in front of Queen Elizabeth when he wore a lucky charm on his dive."

Vanuatu is one of the most traditional countries in the South Pacific, and superstitions remain a cornerstone of the culture. Even though most of the islanders are Christian, ancient Vanuatu beliefs still prevail. This was evident the day of the ceremony when many of the locals abandoned their western dress for more traditional garb?penis sheathes or grass dresses.

The morning of the Naghol I awoke to drum beats. The diving wouldn't begin until the afternoon, but already the day's festivities were starting. At noon a village performed a play about a good spirit overcoming a mud-covered demon. Later, during an elaborate village lunch, string and drumming bands played their latest music.

At 2 p.m. it was time for the land diving. Everyone migrated to the jump site outside the village. I followed closely behind the female dancers. According to tradition, only men are allowed to dive, but female dancers play an essential part by giving emotional and spiritual support.

The first diver climbed the tower and greeted the crowd like a rock star?the spectators roared back. Underneath the tower other jumpers softened the soil with long sticks in case a diver accidentally crashed.

### Defying Death

After playing to the audience some more, the boy leapt headfirst toward the ground. The vines were six inches too long and he smacked the earth hard with his head. Amazingly, he was OK. His friends quickly grabbed him, stood him up,



and freed his ankles. When he walked away, the boy raised his arms to the crowd and waived. More cheering ensued. The dangerous ritual repeated itself until the fifth dive, when a teenager's jump went terribly wrong. On the way down the teenager's vines snapped and the young boy slammed face first into the earth. I saw his lips and nose covered in dirt. Everyone rushed to his aid. But the teenager was unconscious.

Someone rushed in with cold water and poured it on his head; immediately the teenager started showing signs of life. After a few minutes of recovery, he was able to limp away, helped by friends and brothers. Everyone breathed a sigh of relief. The dancing and land diving continued.

To rekindle the festive mood one of the elders decided to jump next. For a long time the experienced diver stood on the tower, building anticipation in the crowd and inviting them to regain their enthusiasm. Eventually he made the leap. It was a perfect dive?he landed safely.

#### Participating in the Dive

After another jump, it was my turn to join the action. It's taboo for foreigners to land dive, but I wasn't going to let that stop my camera from doing it. After a lot of explaining, the chief finally granted me permission to tie a small camera to a diver's leg. It was an unprecedented moment. The crowd loved it, despite the fact that some questioned whether it was taboo-like.

Once the camera was recording, the young man climbed the tower. At the top, vines were attached to his legs. After a few moments of greeting the crowd, he made the dive. Thankfully, he landed safely?or I was going to have 300 angry people chasing me through the jungle. I rushed into the jump area and greeted the diver. The camera had caught it all.

In the late afternoon, the last jump took place. The finale was by one of the most experienced divers in Pentecost. He's been diving for decades. He lunged from the tower confidently and landed safely. Everyone went wild. When it was over, young kids swarmed the tower, dreaming of the day when they would be old enough to also be land divers.

In the evening, a pig was cooked to celebrate a successful Naghol. The string band emerged and many of the young people started dancing. As I downed my kava while watching the festivities, I couldn't help but think about those perilous land dives. The Naghol is Pentecost's culture in its most daring form?men defying death in an unusual and spectacular way.

National Geographic Today, 7 p.m. ET/PT in the United States, is a daily news

journal available only on the National Geographic Channel. Click here to learn more about it.

| 239|2002-12-01 15:50:32|TTT UUU|Re: Fwd: Ancient map of Africa from China poses questions|

Just did a Yahoosearch and there is a lot of material about Hapgood. He published another book in 1970 about the moving earthpoles and died in 1982. Lots of reading for members interested in ancient history.

The maps Hapgood researched are extant and were scientifically investigated. All the ancient authors have survived in copied manuscripts. I consider Maps of the Ancient Sea Kings to be one of the most interesting books I have picked up. Highly recommended.

**TTT UUU** wrote:

Paul, the whole thrust of Hapgood's book is about these mysterious peoples he calls Ancient Sea Kings, who left us these copies of incredible maps that show parts of north Europe at the end of the last glaciation, and parts of Antarctica (Queen Maud's Land) exposed from existing ice (pre-ice age). Yes the De Canerio map is clearly dated in the text from 1502 but Hapgood's evidence points to the antiquity of the original data. (All the ancient works survive as copied/alterd manuscripts). These maps vastly predate Ptolemaic Egypt (approx. 100BC-200AD) and the Chinese Shang dynasty (1,400 BC). The Chinese were not mapping the world prior to and just after the last ice age now were they? You and I and the rest of the world do not know exactly what groups produced these very old maps but Hapgood coined The Sea Kings name and we need to follow this lead.

Hapgood explains the problem of calculating longitude very clearly.

Essentially, historians have to readjust their thinking. As an example agriculture was not originally invented in Iraq and Iran. Author Joseph Campbell suspected it was much older and probably involved tropical root crops in SE Asia and Africa but these roots do not preserve well in the heat. The continent of SE Asia prior to sea floods at 7,500 BC probably had numerous human communities involved in agriculture, sea trading, astronomy and megalithic architecture. We will have to wait for more submarine excavations/explorations.

We really must follow up on what people made of Hapgood's book after it was released in 1966. Perhaps other academics trashed it with justification. I don't know yet. If we accept Hapgood's main thesis then intelligent human history is very old indeed, a lot of knowledge has been lost and all the history books have to be re-written.

Comments please.

*a.manansala@attbi.com* wrote:

But the De Canerio map dates only to about 1502 not 1389 like the Chinese map.

It may be that there was an ancient Alexandrian source for these maps but we do not have any extant evidence. The use of Alexandria as a meridian may extend back only to Ptolemy who made his own observations from this city (he was an Upper Egyptian).

Regards,  
Paul Kekai Manansala

>

> I have Hapgood's book in front of me this morning. Oops! I should have said De

> Canerio (map) instead of "Cantino" in my last post. Page 117, etc. states

> Hapgood's ideas of the very ancient source of the very accurate map of Africa.

> The Portolan (Port to Land) maps he shows are extremely accurate and he

> concluded they were copies from ancient sources. They could not be created in

> Europe at the time as the math did not exist. His book is well worth reading in

> detail. [He was mistaken about the age of the Cucillo pyramids, Mexico. These

> have been dated more recently,]

> Hapgood coins a name for the unknown map source in the title of his book Maps

> of the Ancient Sea Kings . Who were the Ancient Sea Kings?

Some other writers

> call this the Elder Culture. Were they Austric, pre-dynastic Egyptian, related

> to the sunken walls and cities of Taiwan-Okinawa and India (off Gudgerat)? We

> need more submarine archaeology on the continental shelves.

> I don't know if Hapgood is still alive. His book was published in 1966. Will do

> some digging.

> Robin Day S. Korea

> TTT UUU wrote:

> I was referring to the Egyptian-Greek Ptolemaic Dynastic period

founded by one of

- > Alexander's generals (the Greek Phaeros). Paul, I believe you refer individually

- > to Cladius Ptolomy (85-165AD) the Greek astronomer or his group of geographers

- > associated with the Library of Alexander at the time. They did produce maps

- > (originals do not survive) and Hapgood refers to the Cantino Planisphere map

- > (and other surviving Mediterranean maps) as examples/copies that derive their

- > accuracy/origin from the group at the Library of Alexander. This was one of

- > Hapgood's main theses. Did Claudius Ptolomy originate the Alexandrian mapping

- > methods or receive the math and maps from earlier peoples?

We don't know. I can

- > reply in more detail tomorrow when I have Hapgood's book in front of me. I just

- > read it in September.

- > Robin Day, S. Korea

- > a.manansala@attbi.com wrote:

- >>

- >> Paul

- >

- >

- >

- >> The Ptolomy-Alexandrian map source (Greek Egyptian dynasty, perhaps 300 BC) is

- >> older than the Chinese silk map and shows a vastly more accurate African

- > outline

- >

- >

- > No, Ptolemy did not leave us any maps and he did not have a good idea of the

- > shape of the continent based on his tables of latitudes and longitudes. His

- > knowledge seemed to stop around present-day Tanzania.

- >

- > Maps derived from his Geographia showed that he even created an error by

- > changing the idea that the continent was surrounded by ocean. Instead, he

- > depicted the Indian Ocean as a massive land-locked lake with Africa extending

> far to the south and connecting with Asia.

>

> Regards,

> Paul Kekai Manansala

>

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| 240|2002-12-02 03:31:43|Richard Wordingham|Is the Palaung grandchild Tai?|  
The entry for 'grandchild' in the Austric glossary indexed from  
<http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm> derives  
Palaung (Panku) hlan3 'grandchild' for a Proto-Austro-Asiatic form  
\*lali, derived from the Austric root \*li. How much confidence is  
there that this word is not a loan from Shan \*lan, or rather the  
earlier Proto-Tai form \*hlan 'grandson, nephew'? (I am not 100% sure  
that 'lan' actually occurs in Shan - I don't have access to a Shan  
dictionary - but it does occur in all 3 Tai groups and cognates occur  
in Sui, Mak and T'en.) Palaung is spoken in the Shan States, and Li  
has found other examples where Palaung corresponds to Proto-Tai  
\*hl-, such as Palaung hla (with a short vowel - should it  
be 'hl&') 'stake' and hlai 'flow'. He doesn't mention 'hlan3'; is  
its range in Palaung restricted? Besides the phonetic similarities,  
other problems are that Palaung is the only language cited with a  
form derived from \*lali, and that there is a large range of meanings  
for the derivatives of Austric \*li.

The Proto-Tai word may well be related to Austric \*li; an Austro-Tai  
form (q)lal[a]  
has been proposed from which Proto-Tai \*hlan might be derived.  
However, I fear the case is weakened if Palaung hlan3 is from Tai.  
The problem lies in the semantics rather than the phonetics, for  
there is evidence for the MK form \*k[a]lili.

I'm not sure how much impact classifying Palaung hlan3 as a loan from  
Proto-Tai would have on the Austric reconstruction; I think it is  
only slight, even though it rule out the best MK-Austronesian match  
for word forms from the root \*li.

Richard.

| 241|2002-12-03 20:37:06|Paul Kekai Manansala|Re: Mon Conch|

--- In austric@y..., LV Hayes wrote:

> >From: "Richard Wordingham"

> >Date: Fri, 29 Nov 2002 09:24:50 -0000

> >Subject: [austric] Mon Conch

> >

> >While looking at the Austric gloss for 'crustacean' in the

> >Austric website at [http://home.att.net/~lvhayes/Langling/](http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm)

> >Glossary/glosspg1.htm , I saw Old Mon saN 'conch' given as

> >a derivative of Proto-Austr(o-Asiat)ic \*zaN. ('N' = velar

> >nasal.) Is there not a strong possibility that the Mon

> >word actually derives from Sanskrit s'aNkha 'conch'? (s'

> >is the palatal sibilant, written 'z' by the Harvard school.)

>

> Yes, Sanskrit s'an.kha is a plausible source of Old Mon

san. 'conch'.

> However, given the demonstrated PMK shift \*/z > s/, it is equally

plausible

> that Old Mon san. reflects retention of PAA \*zang. I

Benedict has Austro-Thai \*qu(n)zang "crustacean, shrimp."

Regards,

Paul Kekai Manasnala

| 242|2002-12-03 21:38:13|Hiep Duc Nguyen|Austric glossary |

Hello all,

I am not a linguist specialist, but i found the Austric glossary composed by LV Hayes quite informative and useful. I would like to add another piece of information in the glossary of "earth" (proto Austric of \*taq, \*teq or proto AA of \*(n)teq, \*(n)taq). As a Vietnamese, i know the word for "earth" in Vietnamese is dat (dda^t). Is this related to the proto Austric word \*taq ?.

Hiep

On Mon, 2 Dec 2002, Richard Wordingham wrote:

> The entry for 'grandchild' in the Austric glossary indexed from

> <http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm> derives

> Palaung (Panku) hlan3 'grandchild' for a Proto-Austro-Asiatic form

> \*lali, derived from the Austric root \*li. How much confidence is

> there that this word is not a loan from Shan \*lan, or rather the

> earlier Proto-Tai form \*hlan 'grandson, nephew'? (I am not 100% sure

> that 'lan' actually occurs in Shan - I don't have access to a Shan

> dictionary - but it does occur in all 3 Tai groups and cognates occur  
> in Sui, Mak and T'en.) Palaung is spoken in the Shan States, and Li  
> has found other examples where Palaung corresponds to Proto-Tai  
> \*hl-, such as Palaung hla (with a short vowel - should it  
> be 'hl&'?) 'stake' and hlai 'flow'. He doesn't mention 'hlan3'; is  
> its range in Palaung restricted? Besides the phonetic similarities,  
> other problems are that Palaung is the only language cited with a  
> form derived from \*lali, and that there is a large range of meanings  
> for the derivatives of Austric \*li.  
>  
> The Proto-Tai word may well be related to Austric \*li; an Austro-Tai  
> form (q)lal[a]  
> has been proposed from which Proto-Tai \*hlan might be derived.  
> However, I fear the case is weakened if Palaung hlan3 is from Tai.  
> The problem lies in the semantics rather than the phonetics, for  
> there is evidence for the MK form \*k[a]lili.  
>  
> I'm not sure how much impact classifying Palaung hlan3 as a loan from  
> Proto-Tai would have on the Austric reconstruction; I think it is  
> only slight, even though it rule out the best MK-Austronesian match  
> for word forms from the root \*li.  
>  
> Richard.  
>  
>  
>  
| 243|2002-12-04 07:34:31|Richard Wordingham|Re: Mon Conch|  
--- In austric@y..., "Paul Kekai Manansala" wrote:  
> --- In austric@y..., LV Hayes wrote:

> > Yes, Sanskrit s'an.kha is a plausible source of Old Mon  
> san. 'conch'.  
> > However, given the demonstrated PMK shift \*/z > s/, it is equally  
> plausible  
> > that Old Mon san. reflects retention of PAA \*zang. I  
>  
> Benedict has Austro-Thai \*qu(n)zang "crustacean, shrimp."

whence Proto-Tai \*kuN and thence Siamese kuN. Do the Austro-Tai  
alternatives explain the tone variation? SW and Central Tai have tone  
C, but Northern Tai has tone B. (For the uninitiated who know the  
Thai script, A, D = no tone mark, B = superscript 1 - mai ek - and C =  
superscript 2 - mai tho.)

I wasn't doubting Proto-Austric \*zaN. It was just that doubtful  
cognates are better marked as such. If you browse the glossary, you  
will see comments about many Proto-Chamic forms, presumably because of



this very principle.

Incidentally, how do we handle the 'exotic' phonemes in extended ASCII on this list? I would normally use displaced diacritics (e.g. for one retroflex) backed up by extended SAMPA, but the latter gets ugly for post-velars and very ugly for implosives (e.g. [b\_ implosive bilabial]). Confusion can still arise - to me has the dot below, so retroflex, but to LV has the dot above, so velar!

Richard.

| 244|2002-12-04 09:28:49|a.manansala@attbi.com|Re: Mon Conch|

> --- In austric@y..., "Paul Kekai Manansala" wrote:  
>> --- In austric@y..., LV Hayes wrote:  
>  
>>> Yes, Sanskrit s'an.kha is a plausible source of Old Mon  
>> san. 'conch'.  
>>> However, given the demonstrated PMK shift \*/z > s/, it is equally  
>> plausible  
>>> that Old Mon san. reflects retention of PAA \*zang. I  
>>  
>> Benedict has Austro-Thai \*qu(n)zang "crustacean, shrimp."  
> whence Proto-Tai \*kuN and thence Siamese kuN. Do the Austro-Tai  
> alternatives explain the tone variation? SW and Central Tai have tone  
> C, but Northern Tai has tone B. (For the uninitiated who know the  
> Thai script, A, D = no tone mark, B = superscript 1 - mai ek - and C =  
> superscript 2 - mai tho.)  
>

Can't say offhand whether Benedict deals with this tone variation. Probably not.

> Incidentally, how do we handle the 'exotic' phonemes in extended ASCII  
> on this list?

Maybe as simply as possible for the uninitiated by capitalizing the exotics and providing explanation when needed. The vowel system in Austro-Asiatic can get pretty complex, and there are some pretty unusual consonant sounds too like the checked consonants.

Regards,

Paul Kekai Manansala

| 245|2002-12-04 15:33:09|TTT UUU|Re: Fwd: Wakwak of Indonesia|

I can find v. little info about. the Wakwak of Malaya. I just get pages of some Japanese website in my netsearch. The Madagascar [www.lemurdolls](http://www.lemurdolls) site mentions the attempted invasion of E. Africa....very interesting indeed!

*a.manansala@attbi.com* wrote:

> It may that the some other knowledge helped the Chinese in their map-making.  
> Obviously that was one reason that I posted the articles here in the first  
> place.  
>

To illustrate this, we can note that the Muslim writings mention in several instances that the people of Zabag and Wakwak in the Malay Archipelago traded on the coast of East Africa. There is even a notice of an attempted invasion of Qanbalu in present-day Tanzania by the Wakwak.

So frequently did these Austronesian mariners visit that they were easily able to communicate with the African peoples that lived there.

This knowledge would have been useful for any Chinese expedition to Africa.

Tupaia probably helped cut down the time it required Europeans to map the Pacific considerably.

However, someone with this type of knowledge would have had a much greater impact in earlier times when long-range navigation was still mostly an unknown art.

And I think it would have been very simple for an Austronesian navigator to quickly understand other types of geographic and astronomical symbolism.

Regards,  
Paul Kekai Manansala

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| 246|2002-12-05 09:15:36|Richard Wordingham|Re: Austric glossary|

--- In austric@y..., Hiep Duc Nguyen wrote:

>

> Hello all,

>

> I am not a linguist specialist, but i found the Austric glossary

> composed by LV Hayes quite informative and useful. I would like to

add

> another piece of information in the glossary of "earth" (proto

Austric of

> \*taq, \*teq or proto AA of \*(n)teq, \*(n)taq). As a Vietnamese, i know

the

> word for "earth" in Vietnamese is dat (dda^t). Is this related to

the

> proto Austric word \*taq ?.

Well, until we can get an expert opinion, here is my inexperienced opinion.

On the basis of the examples gio.t < PAA \*jatuq 'drop' and 璫 < PAA  
\*taki 'lead' in the glossary, I would say that 𪔐 < PAA \*\*tataq  
'earth' certainly looks plausible. The only problems I can see are  
that:

1. The use of the prefix /ta/ is not otherwise attested with the root  
\*taq, \*teq, though there are plenty of examples of the prefix \*ta(n)-.  
(I would not count Tariang taneh 'earth, land' as an example of this  
prefix, though unfortunately one search engine for a Bahnaric database  
does.)

2. The vowel

more examples of how PAA CaCag developed before we could make a final judgement.

However, I am no expert on Vietnamese or Austro-Asiatic. I knew next to nothing about the Austro-Asiatic until last month, and very little about Vietnamese.

Richard.

| 247|2002-12-05 10:45:57|a.manansala@attbi.com|Fwd: 5,000 year-old coffins unearthed in Taiwan|

Taipei Times

<http://www.taipeitimes.com/News/taiwan/archives/2002/12/04/185833>

Archaeologists unearth wooden coffins

FANTASTIC FIND: Archaeologists working at a dig site in the Tainan Science-based Industrial Park have discovered a 5,000-year-old coffin and the skeletons of a couple

CNA

"Each of [the wooden coffins] was 40cm long and 10cm wide. They are made of hardwood and are dark brown in color. We need further examination to determine the exact type of wood."

Chu Cheng-yi, research fellow with the Institute of History and Philology of Academia Sinica

A wooden coffin believed to be nearly 5,000 years old has been unearthed at an archaeological site in the Tainan Science-based Industrial Park in southern Taiwan, a local archaeologist said yesterday.

"We discovered the grave at a site reserved for construction of a public facility in the science park on Monday," said Chu Cheng-yi (????y), a research fellow with the Institute of History and Philology of Academia Sinica -- Taiwan's highest academic body.

Chu, who has been conducting a field study at the science park since 1995, said it was the first time his archaeological team had discovered a grave with a wooden coffin in the area.

In the past, the team has found stone sarcophagi, but nothing like these wooden coffins.

The team unearthed two wooden coffin slabs, Chu said.

"Each of them was 40cm long and 10cm wide," he explained. "They are made of hardwood and are dark brown in color. We need further examination to determine

the exact type of wood," he said.

Also unearthed in the grave were more than 20 cord-marked pots and many pottery shards.

Archaeologists also discovered the skeletons of a man and a woman, both of whom were in their 20s when they died, Chu went on.

"This is also the first time that we have discovered the remains of a couple buried together in a single grave in the region," Chu told reporters.

Over the past six years, Chu's team has unearthed more than 370 human skeletons and a large quantity of pots and shards at nine archaeological sites in the Tainan science park, which is still under construction.

The newly discovered grave was found beneath the ninth archaeological site, known as the Nankuanli East cultural relic.

According to Chu, his team last month also excavated a grave containing 20 human skeletons, two complete dog skeletons, 140 rubbish pits, as well as many grains and shells at the site.

"All of the skeletons and artifacts were buried at a depth of between 0.5m and 1m in an area spanning 1,500m<sup>2</sup>," Chu said.

He added that the relics belong to the Tapenkeng culture which dates back 5,000 years.

Construction at the Tainan science park will shift into high gear next year.

Chu said his team must step up its work to rescue as many cultural relics in the region as possible before they are covered by concrete for the construction of the site.

| 248|2002-12-05 15:57:46|LV Hayes|Re: Origin of Tai Voiceless Aspirates|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>

>Date: Fri, 29 Nov 2002 10:00:17 -0000

>Subject: [austrie] Re: Origin of Tai Voiceless Aspirates

>

>BTW, how do you categorise Li's reconstruction of Proto-Tai

>\*dl and \*dr from patterns of apparent reflexes of \*r- and

>\*l-? Divine inspiration?

I'd categorize it as an excellent example of the comparative analysis that a competent comparatist can undertake in such studies and a fine demonstration of what the historical linguist can do with a given set of

sparse facts available to him. Li's reasons for doing what he did are set out on pages 124ff. and 128 of his *Handbook*, and they seem adequately clear to me.

>I was just thinking of the immediate issue that Li raises,  
>namely that sometimes the Sui cognates start with a  
>preaspirated nasal and sometimes with a preglottalised nasal.  
>Li gives examples of the contrast (not minimal pairs) when  
>justifying the reconstruction of Proto-Tai *\*ʔb*.

The clusters /hm/ and /ʔm/ appear initially in a number of SEAsian languages, including some belonging to the MK subfamily of AA. Experience gained in my own personal studies indicates that both derive from earlier *\*/Cm/* (often from still earlier *\*/CVm/*), where *\*C* represents two types of proto-consonants, one of which shifted to /h/, the other to /ʔ/ (ʔ represents the glottal stop or feature of the cluster).

It is difficult to say precisely what the *\*/C/* was in any given environment, lacking cognates in which the consonant has been retained, but it appears that as a general rule, occlusives > /ʔ/ and continuants > /h/.

>Are the examples in Gage (1985, Glottal Stops and Vietnamese  
>Tonogenesis, in 'For Gordon H. Fairbanks', ed. V.Z.Acson &  
>R.L.Leed, Oceanic Linguistics Special Publication No. 20,  
>University of Hawaii Press, pp21-36) that Thurgood cites in  
><http://www.csuchico.edu/~gt18/Papers/Vietnamese&tonogenesis.pdf>  
>in this category?

Thanks for the references; I hadn't seen the Gage or Thurgood papers before. I've downloaded Thurgood's paper and will take a look at it. Do you have any idea when this was written and/or published?

>Do we need /ʔm/ as opposed to /mʔ/? Li writes that the final  
>glottal stop that is a feature of the low falling (21) tone of  
>the Lungchow dialect follows rather than precedes the otherwise  
>final nasal.

It depends on the phonological facts of the individual language. Phonetic descriptions of the Viet tones indicate that the glottalization typically occurs on the nucleic vowel, such that [aʔam] or [aʔm] would most closely represent what is taking place phonetically. In other languages, such as the Lungchow dialect you mention, things would be different, of course.

My comment was to indicate that if a glottalized final consonant occurs, it

is likely to derive diachronically from \*/mVq > mV?/, whence \*/m?/ and/or \*/?m/. I do not think that glottalized consonants were original to any of these languages.

>>Even from the first, the affiliation of \*(ka)?duuk was  
>>dubious, though Benedict's inclusion of Kam-Sui \*/?dlaak  
>>and Proto-Kam-Sui \*tla:k7 'bone' offered a plausible  
>>bridge between the forms.

>

>Do you actually mean tl > ?dl for Kam-Sui with a parallel  
>change for Tai? (The best evidence for \*/?dl that Li found  
>in Tai was in Saek, which Ethnologue (why?) excludes from  
>Tai.) If this is what happened, I can have no confidence  
>in my witchcraft!

No, I am simply trying to describe what Benedict said about the matter in his AT book, which apparently you don't have at hand. I have no intentions of trying to argue pro or con with respect to historical changes in Kam-Sui. As stated before, I do not have the knowledge, resources, time, or desire to engage in such discussions. The phonology of these languages is extremely complicated, and the novice who dares comment without adequate background is taking excessive risks.

As for the affiliation of Saek, I don't remember what I used to know about this language. I'm thinking that 2 languages of same or similar name from the same general area got confused; one of which turned out to be Tai (or Daic) and the other which turned out to be MK (either Khmuic or Vietic). Don't quote me on that, however!

>>My latest Proto-AA form (see web site) is \*c(i,a,u)(n)qang  
>>'bone' ...

>

>I'm not sure I understand your notation. I take it round  
>brackets indicate a multiple choice or, given only one  
>option, an optional part, but what do square brackets mean?

I thought I had it explained somewhere, but maybe not. Round brackets or parentheses indicate optionality, square ones possibility or uncertainty. Thus, the 'bone' proto-form indicates that the morphological variants \*ciqang, \*cinqang, \*caqang, \*canqang, \*cuqang, \*cunqang may occur. \*q[a]ng would indicate that the nucleic vowel might have been \*a or something else.

>>One must search and research the lexicons and examine and  
>>re-examine the data before one spies that bit of gold  
>>glittering in the mud.

>

>And beware the iron pyrites!

Sure, you'll find some fool's gold and your readers will probably find even more for you.

>>Note that the d of the MK forms corresponds to t in Paul

>>Sidwell's Proto-South Bahnaric \*n@ting 'bone'.

>

>The archives have mangled n @ t i n g; on cybalist we use '&'

>instead of '@' so as to avoid this problem.

I should have noted that the @ in \*n@ting denotes shwa. I'm not aware of the "mangled" issue, and @ is used for shwa in many e-fora, usually with an explanation.

>He has stripped off the classificatory prefixes that are

>independent words, such as plaa 'fish' and 'nok' (etc.)

>'bird'.

The prefixes I had in mind were those which are phonologically bound to the glosses, such as ?@ in Souei ?@kaa 'fish'.

LV Hayes

| 249|2002-12-05 17:02:19|LV Hayes|Re: Austric glossary|

>From: Hiep Duc Nguyen <[hduc@airmoon.epa.nsw.gov.au](mailto:hduc@airmoon.epa.nsw.gov.au)>

>Date: Wed, 4 Dec 2002 17:44:22 +1100 (EST)

>Subject: [austric] Austric glossary

>

>I am not a linguist specialist, but i found the Austric

>glossary composed by LV Hayes quite informative and useful.

>I would like to add another piece of information in the

>glossary of "earth" (proto Austric of \*taq, \*teq or proto

>AA of \*(n)teq, \*(n)taq). As a Vietnamese, i know the word

>for "earth" in Vietnamese is dat (dda^t). Is this related

>to the proto Austric word \*taq ?.

As you'll note from a review of the 'earth' etymology in the Austric glossary, there is no Vietnamese correspondent present. This is not because there are no possibilities. In fact, there are two. But in every case, the connections, if they exist, are excessively fuzzy.



Viet ddi.a 'earth, land' is in my view the real Vietnamese correspondent to PAA \*(n)teq, but it is per the dictionaries a Sino-Vietnamese form restricted to usage in compounds. Thus, it is of Chinese origin and not to be used in such etymologies.

Well, I have my doubts about that history. I've found a number of Sino-Viet words which would appear quite well explained as inherited from Austric and not as borrowed from Chinese. In fact, I've seen some which I suspect Chinese borrowed long ago from an Austric language and then later loaned to Vietnamese (or other SEASian languages).

The short form of the story is that it's not easy to argue when something is pronounced as being of Chinese origin. Nobody is going to believe you, and it's likely that the Chinese origin is going to have better facts in favor of it than any that you're going to find and bring in as counterproof. So, ddi.a does not appear in the 'earth' etymology.

To get back to dda^t, this does appear to be a bonafide Vietic word with no suspicion of Chinese origins. But if dda^t descends from a form related to PAA \*(n)teq, the linkage certainly doesn't jump out and grab you. Hence, it's not cited in the 'earth' etymology, either.

If one studies the 'earth' etymology, one notes that there are several distinct paths of evolution from \*(n)teq, with much apparent irregularity in the reflexes of the ancient final. It does appear possible, at least to me, that dda^t could be a reflex of \*(n)teq, but I am hardly certain which of those paths, if any, this reflex took to eventually become dda^t. Thus, the possibility exists, but the degree of uncertainty is so high that prudence rules in favor of leaving the matter out of the 'earth' etymology.

LV Hayes  
| 250|2002-12-05 19:42:26|Hiep Duc Nguyen|Re: Austric glossary|  
Hello LV Hayes, Richard Wordingham and all,

Many thanks for your explanation. You are quite knowledgeable in Vietnamese and other languages.

On the issue of Chinese borrowing some of the Austric words, raised by LV Hayes, it is quite interesting for me. A friend of mine told me that the concept and the 12 animal symbols in the Chinese zodiac were actually derived from the people in the South called Yueh (Austric or Thai ?). For example, he explained the word "horse" in Chinese (ma~) as come from Yueh (as i am in the office, i don't have the article in hand). But i am not so sure about this.

W. Meacham in one of his papers mentioned that the civilization of early

China in the Shang and Chou periods had many contacts and contributions from the Yueh people (Origins and development of the Yueh Coastal Neolithic: A microcosm of cultural change on the mainland of East Asia, released in 1981). So it is very feasible that China could borrow a number of Austric words long ago.

Cheers,  
Hiep

On Thu, 5 Dec 2002, LV Hayes wrote:

> >From: Hiep Duc Nguyen <[hduc@airmoon.epa.nsw.gov.au](mailto:hduc@airmoon.epa.nsw.gov.au)>  
> >Date: Wed, 4 Dec 2002 17:44:22 +1100 (EST)  
> >Subject: [austric] Austric glossary  
> >  
> >I am not a linguist specialist, but i found the Austric  
> >glossary composed by LV Hayes quite informative and useful.  
> >I would like to add another piece of information in the  
> >glossary of "earth" (proto Austric of \*taq, \*teq or proto  
> >AA of \*(n)teq, \*(n)taq). As a Vietnamese, i know the word  
> >for "earth" in Vietnamese is dat (dda^t). Is this related  
> >to the proto Austric word \*taq ?.  
>  
> As you'll note from a review of the 'earth' etymology in the Austric  
> glossary, there is no Vietnamese correspondent present. This is not  
> because there are no possibilities. In fact, there are two. But in every  
> case, the connections, if they exist, are excessively fuzzy.  
>  
> Viet ddi.a 'earth, land' is in my view the real Vietnamese correspondent to  
> PAA \*(n)teq, but it is per the dictionaries a Sino-Vietnamese form  
> restricted to usage in compounds. Thus, it is of Chinese origin and not to  
> be used in such etymologies.  
>  
> Well, I have my doubts about that history. I've found a number of  
> Sino-Viet words which would appear quite well explained as inherited from  
> Austric and not as borrowed from Chinese. In fact, I've seen some which I  
> suspect Chinese borrowed long ago from an Austric language and then later  
> loaned to Vietnamese (or other SEASian languages).  
>  
> The short form of the story is that it's not easy to argue when something  
> is pronounced as being of Chinese origin. Nobody is going to believe you,  
> and it's likely that the Chinese origin is going to have better facts in  
> favor of it than any that you're going to find and bring in as  
> counterproof. So, ddi.a does not appear in the 'earth' etymology.  
>  
> To get back to dda^t, this does appear to be a bonafide Vietic word with  
> no suspicion of Chinese origins. But if dda^t descends from a form

> related to PAA \*(n)teq, the linkage certainly doesn't jump out and grab  
> you. Hence, it's not cited in the 'earth' etymology, either.  
>  
> If one studies the 'earth' etymology, one notes that there are several  
> distinct paths of evolution from \*(n)teq, with much apparent irregularity  
> in the reflexes of the ancient final. It does appear possible, at least to  
> me, that dda^t could be a reflex of \*(n)teq, but I am hardly certain which  
> of those paths, if any, this reflex took to eventually become dda^t.  
> Thus, the possibility exists, but the degree of uncertainty is so high that  
> prudence rules in favor of leaving the matter out of the 'earth' etymology.

> LV Hayes

| 251|2002-12-05 21:38:49|LV Hayes|Is the Palaung grandchild Tai?|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>

>Date: Mon, 02 Dec 2002 11:31:40 -0000

>Subject: [austric] Is the Palaung grandchild Tai?

>

>The entry for 'grandchild' in the Austric glossary indexed  
>from

><http://home.att.net/~lvhayes/Langling/Glossary/glosspg1.htm>

>derives Palaung (Panku) hlan3 'grandchild' for a Proto-

>Austro-Asiatic form \*lali, derived from the Austric root

>\*li. How much confidence is there that this word is not a

>loan from Shan \*lan, or rather the earlier Proto-Tai form

>\*hlan 'grandson, nephew'? (I am not 100% sure that 'lan'

>actually occurs in Shan - I don't have access to a Shan

>dictionary - but it does occur in all 3 Tai groups and

>cognates occur in Sui, Mak and T'en.) Palaung is spoken in

>the Shan States, and Li has found other examples where

>Palaung corresponds to Proto-Tai \*hl-, such as Palaung

>hla (with a short vowel - should it be 'hl&'?) 'stake' and

>hlai 'flow'. He doesn't mention 'hlan3'; is its range in

>Palaung restricted? Besides the phonetic similarities,

>other problems are that Palaung is the only language cited

>with a form derived from \*lali, and that there is a large

>range of meanings for the derivatives of Austric \*li.

>

>The Proto-Tai word may well be related to Austric \*li; an

>Austro-Tai form (q)lal[a] has been proposed from which

>Proto-Tai \*hlan might be derived. However, I fear the case

>is weakened if Palaung hlan3 is from Tai. The problem lies

>in the semantics rather than the phonetics, for there is

>evidence for the MK form \*k[a]lili.

>

>I'm not sure how much impact classifying Palaung hlan3 as a

- >loan from Proto-Tai would have on the Austric reconstruction;
- >I think it is only slight, even though it rule out the best
- >MK-Austronesian match for word forms from the root \*li.

Palaung (Panku) hlan3 'grandchild' could very well be a Thai borrowing; I have no other MK form of this type, though Pearic sling 'elder sibling' comes close. And removing Palaung (Panku) hlan3 from the cognate inventory would have, I think, absolutely no impact on the viability of the 'grandchild' etymology.

As for the nature of the earlier Tai-Kadai form, note the Kam-Sui \*qhlaan 'grandchild' and Li \*[ql]aal cited by Benedict (1975:303). The Li reconstruction may suggest that \*lala was the earliest form in this language family.

LV Hayes

| 252|2002-12-06 04:41:47|LV Hayes|Re: Mon Conch|

- >From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>
- >Date: Wed, 04 Dec 2002 15:34:27 -0000
- >Subject: [austric] Re: Mon Conch
- >
- >>Yes, Sanskrit s'an.kha is a plausible source of Old Mon
- >>san. 'conch'. However, given the demonstrated PMK shift
- >>\*/z > s/, it is equally plausible that Old Mon san.
- >>reflects retention of PAA \*zang.
- >
- >Benedict has Austro-Thai \*qu(n)zang "crustacean, shrimp."
- >whence Proto-Tai \*kuN and thence Siamese kuN. Do the
- >Austro-Tai alternatives explain the tone variation? SW and
- >Central Tai have tone C, but Northern Tai has tone B.

My interest in tone evolution has been focussed on the tone history of Vietnamese. I have no idea how tonal variation has developed in Tai-Kadai. Those who have tried, e.g. Haudricourt and Ferlus, have not had much success explaining how words ending in nasal or other non-stop finals acquired in Vietnamese tones whose evolution is associated with stop finals. Well, they would explain it as the result of a series of glottalized finals, but there's little evidence for such a series in other MK languages.

You're correct in that Benedict proposed AT \*qu(n)zang 'crustacean, shrimp', but he did not cite Proto-Tai \*kuN or Siamese kuN in association with that proto-form in his 1975 book (see page 260); I assume you're not

aware of this fact. The only Tai-Kadai forms he mentioned were Ong Be zoang 'shrimp, lobster' and WS Li fiang 'shrimp', the former possibly from \*[q/](u)dang > \*[?](u)duang per Benedict, the latter possibly from \*h(u)rang > \*hrwang > \*fuang. The 'crustacean' etymology is not cited in Benedict's 1990 book (\_Japanese/Austro-Tai\_).

>I wasn't doubting Proto-Austic \*zaN. It was just that  
>doubtful cognates are better marked as such. If you browse  
>the glossary, you will see comments about many Proto-Chamic  
>forms, presumably because of this very principle.

I agree. The reason the Mon form was not recognized as a possible Sanskrit loan is because I had simply overlooked the fact that Harry L. Shorto had identified it as such, cf. page 357 of \_A Dictionary of the Mon Inscriptions from the Sixth to the Sixteenth Centuries\_. Actually, one could infer from the way that Shorto lists this entry that san. 'conch' is the Old Mon form and NOT borrowed while kanu san. is the Middle Mon version and borrowed from Sanskrit or Pali. That may be why I didn't list the Sanskrit and Pali forms in my comparative file, but on re-examination, it appears doubtful to me that san. could be borrowed in the one case and not in the other, even though it might be possible due to the form's plausible descent from PAA \*zang. Deciding what is or is not borrowed in the SEAsian languages can be very difficult.

Now that I do know the Mon/Sanskrit relationship, I will add a remark to the online etymology. It may take me a while to get around to it, though.

>Confusion can still arise - to me has the dot below, so  
>retroflex, but to LV has the dot above, so velar!

Agreed about the confusion. I would write retroflex n as n. as well, so best to use either an explanatory note or a special symbol explained in a note. What ASCII system are you interested in using? The last time I looked at this matter ... several years ago ... there were several ASCII systems available. A professor from Oregon had developed a very formal system, but I don't recall his name or web address at the moment.

Actually, Unicode should permit us to use the expanded linguistic characters in this environment, if we use the right font and have the software to handle Unicode. I normally use the Windows Courier font in my e-mailer, and this font does not have a Unicode extension permitting it to use the linguistic characters. Maybe we can test a font that does have this capability. I normally use the Times New Roman font in the manuscripts of my studies, and this font has the Unicode extension. Of course, the problem is that others not using the font would be unable to

read any message unless they also changed their e-mailers to that font. An ASCII system may still be preferable.

LV Hayes

| 253|2002-12-06 05:15:08|LV Hayes|Re: Austric glossary|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>  
>Date: Thu, 05 Dec 2002 17:15:32 -0000  
>Subject: [austric] Re: Austric glossary  
>  
>Well, until we can get an expert opinion, here is my  
>inexpert opinion.

Expert on what? Benedict was the only expert on all of these languages, and he is dead. Posthumously, some say that he wasn't as expert on everything as previously thought, but I'll argue that he came closer to being an expert on all SEAsian languages as anyone ever has. And I know of no one who can fill his shoes.

>On the basis of the examples gio.t <PAA \*jatuq 'drop' and  
>琫 <PAA \*taki 'lead' in the glossary, I would say that  
>𪔐𪔐 <PAA \*\*tataq 'earth' certainly looks plausible. The  
>only problems I can see are that:  
>  
>1. The use of the prefix /ta/ is not otherwise attested with  
>the root \*taq, \*teq, though there are plenty of examples of  
>the prefix \*ta(n)-. (I would not count Tariang taneh 'earth,  
>land' as an example of this prefix, though unfortunately one  
>search engine for a Bahnaric database does.)

Forms of the taneh type are thought to include the infix /an/, so the base (unaffixed) form was \*teh.

A possibility you've overlooked is partial reduplication. It appears that reduplication is a very old morphological process in the Austric/AT languages. Thus, \*taq could become \*taqtaq (full redupl.) or \*tataq (partial redupl.). Also, \*teq --> \*teqteq or \*teteq. Either \*tataq or \*teteq could plausibly evolve to \*t@tVq, whence \*nt@t > \*n'd@t > Viet dda^t. The shift of \*a or \*e to \*@ would probably involve stress shift, with the change to shwa reflecting shift to the final syllable and loss of this syllable reflecting shift to the first syllable.

Such Gedanken experiments are useful in developing one's analytical and comparative skills. Dempwolff used "what if" methodology in his development of Proto-Austronesian. I found a similar procedure quite

useful in discovering lexical evidence for Austric. One imagines what a modern reflex would look like if it were descended from an Austric proto-form or what the proto-form would look like if it were the antecedent of a modern form; then, one looks for the imagined forms or anything similar to them. The results were amazing at times, heartbreaking at other times.

LV Hayes

| 254|2002-12-06 10:05:56|Richard Wordingham|Re: Austric glossary|

--- In austric@y..., LV Hayes wrote:

>>From: "Richard Wordingham"

>>Date: Thu, 05 Dec 2002 17:15:32 -0000

>>Subject: [austric] Re: Austric glossary

>>1. The use of the prefix /ta/ is not otherwise attested with  
>>the root \*taq, \*teq, though there are plenty of examples of  
>>the prefix \*ta(n)-. (I would not count Tariang taneh 'earth,  
>>land' as an example of this prefix, though unfortunately one  
>>search engine for a Bahnaric database does.)

>

> Forms of the taneh type are thought to include the infix /an/, so

the base

> (unaffixed) form was \*teh.

This database records presyllables, but not infixes or suffixes.  
Perhaps I'm confounding presyllables and prefixes, but the database  
(at <http://starling.rinet.ru/intrtext.htm>) also seems to confound  
them. Treating 'neh' as the 'root', albeit as an option to consider,  
is highly confusing.

> Thus, \*taq could become \*taqtaq (full redupl.) or \*tataq  
> (partial redupl.). Also, \*teq --> \*teqteq or \*teteq. Either

\*tataq or

> \*teteq could plausibly evolve to \*t@tVq, whence \*nt@t > \*n?d@t >

Viet

> dda^t.

Does the 'whence' include prefixing 'n-'?

> Such Gedanken experiments are useful in developing one's analytical

and

> comparative skills. Dempwolff used "what if" methodology in his  
> development of Proto-Austronesian. I found a similar procedure

quite

> useful in discovering lexical evidence for Austric. One imagines

what a

> modern reflex would look like if it were descended from an Austric  
> proto-form or what the proto-form would look like if it were the

antecedent

> of a modern form; then, one looks for the imagined forms or anything  
> similar to them. The results were amazing at times, heartbreaking

at other

> times.

Does the relatively low number of Munda-Austronesian matches reflect  
your search strategy? I don't recall seeing any roots with a Munda  
cognate but not an MK cognate.

Richard.

| 255|2002-12-06 10:40:13|a.manansala@attbi.com|Fwd: Mexican Skull May Explain Indigenous  
Origins|

Notice the Pacific route theory appears again.

---

Mexican Skull May Explain Indigenous Origins

Thu Dec 5,12:31 PM ET Add Science - Reuters to My Yahoo!

MEXICO CITY (Reuters) - A 13,000-year-old skull found in Mexico may help prove  
theories that some of the New World's first settlers arrived along a Pacific  
Coast route from Japan, and not just across the Bering Strait.

The skull is believed to be the oldest ever discovered in the Americas and is  
among 150 mostly undated specimens being studied by Silvia Gonzalez, a leading  
world authority on prehistoric man and mammoths, after being gathered at  
Mexico's National Museum of Anthropology and other museums.

The skull has the long, narrow-headed cranial features common to the native



peoples of Central and South America, as opposed to the short and broad-headed type characteristic of North American Indians. Gonzalez, with the help of a laboratory in Britain, has determined it is 13,000 years old.

Prevailing theory has it that migrations to the New World came in successive waves across the Bering Strait land bridge that joined northeast Asia and modern Alaska, although recent evidence has emerged that migrations may also have come along the Pacific coast from Japan.

Gonzalez said she plans to take DNA samples on the skull, as well as others, to see if they can help identify where the native people of Mexico and Central America first migrated from.

She said the skull is similar to others found belonging to the now extinct Pericues people who populated the southern tip of Mexico's Baja California state, along the Pacific Coast route, until the 18th century.

"The question is, we have these very ancient individuals, but where did they come from?" said Gonzalez, an earth sciences lecturer at Liverpool's John Moores University in England.

"Are these ones that we find right here in the basin of Mexico coming from the north, from the Bering Strait, or are they coming from the south, heading north toward Mexico?"

The Pacific Coast theory is supported in part by the discovery recently of the oldest confirmed site of human habitation in the Americas, located in Monte Verde, Chile, and dating back 14,000 years.

Gonzalez said the ancient skulls discovered in Mexico may provide more evidence to support that theory.

"The thing is that there are about 150 skeletons in the National Museum of Anthropology in Mexico awaiting study. So that's the next step as well, trying to go through that collection ... and see if there is really scientific support for this coastal route of migration," she said.

| 256|2002-12-07 01:00:58|LV Hayes|Re: Austric glossary|

>From: "Richard Wordingham" <[richard.wordingham@mbda.co.uk](mailto:richard.wordingham@mbda.co.uk)>  
>Date: Fri, 06 Dec 2002 18:05:50 -0000  
>Subject: [austric] Re: Austric glossary  
>  
>>Forms of the taneh type are thought to include the infix  
>>/an/, so the base (unaffixed) form was \*teh.  
>  
>This database records presyllables, but not infixes or  
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>prefixes, but the database (at  
><http://starling.rinet.ru/intrtext.htm>) also seems to  
>confound them. Treating 'neh' as the 'root', albeit as an  
>option to consider, is highly confusing.

Sergei Starostin's Tower of Babel project at the address you cite includes a large number of etymological databases. I visited the site and noted that since the last time I was there, a Bahnaric database by Ilya Pejros has been added. I assume this is the database to which you refer.

Pejros's Bahnaric citations include diacritics that I do not understand, and I found no legend to them in a brief search of the site. It's a waste of time to try to work with language data written in a representational system that one does not understand.

>>Thus, \*taq could become \*taqtaq (full redupl.) or \*tataq  
>>(partial redupl.). Also, \*teq --> \*teqteq or \*teteq.  
>>Either \*tataq or \*teteq could plausibly evolve to \*t@tVq,  
>>whence \*nt@t > \*n?d@t > Viet dda^t.  
>  
>Does the 'whence' include prefixing 'n-'?

Yes, it would, if the n- were necessary. I was trying to explain how \*/t/ became \*/?d/ and overcomplicated things when I forgot that \*/t > ?d/ is a regular change at a certain stage of Vietnamese. Accordingly, the n-element is not needed.

>Does the relatively low number of Munda-Austronesian  
>matches reflect your search strategy? I don't recall  
>seeing any roots with a Munda cognate but not an MK cognate.

The Austric glossary contains all the AA/AN comparisons published in my various articles. The purpose of publishing these comparisons was to provide lexical proof of the Austric unity. Accordingly, I tried to select

comparisons for publication which had both MK and Munda cognates and thus a higher probatory value. Offhand, I don't recall if there is an etymology in the glossary lacking a MK cognate. I do know that I have some comparisons on file in which only a Munda-AN correlation appears.

As for your question, the paucity of Munda-AN matches probably reflects foremost the paucity of Munda lexical materials available to search. Few Munda dictionaries exist, and most of them are not huge. As a consequence, many more Munda-AN matches may exist, but remain to be identified by a more thorough search of more copious Munda lexical materials once the latter are available.

LV Hayes

| 257|2002-12-07 09:12:51|LV Hayes|Re: Austric glossary|

>From: Hiep Duc Nguyen <[hduc@airmoon.epa.nsw.gov.au](mailto:hduc@airmoon.epa.nsw.gov.au)>  
>Date: Fri, 6 Dec 2002 15:48:15 +1100 (EST)  
>Subject: Re: [austric] Re: Austric glossary  
>  
>A friend of mine told me that the concept and the 12 animal  
>symbols in the Chinese zodiac were actually derived from the  
>people in the South called Yueh (Austric or Thai ?).

As I recall, the Chinese term Yueh and the Vietnamese word Vie<sup>^</sup>.t 'Vietnamese' are thought to descend from the same word. This word is believed to have meant a type of axe. It is also thought that this axe was associated with the original Austroasiatic peoples, and its presence has been used to distinguish that folk from other ethnic groups at archaeological sites in southern China.

It would take much reading of available area histories and archaeological studies to get the full picture of the Yueh/Chinese/Vietnamese relationship. Before the Chinese conquered what is now southern China circa 200 BC, the Chinese called the people of southern China the '100 Yueh', which is the same as the Vietnamese or rather Sino-Vietnamese term Ba'ch Vie<sup>^</sup>.t, because there was so many different ethnic groups speaking so many different languages in that region.

>So it is very feasible that China could borrow a number  
>of Austric words long ago.

The historical truth is unclear, but it is probable that Chinese has been in contact with the Austric languages since the time that Chinese first became a separate language. In fact, one linguist (Laurent Sagart) has proposed that Chinese is an Austric language or at least a mixture of

Austric and Sino-Tibetan that evolved around 5,000 or more years ago. The proposal has not been universally well accepted, but what I have seen of it seems very convincing to me.

LV Hayes

| 258|2002-12-09 07:46:17|Richard Wordingham

--- In [austric@yahoogroups.com](mailto:austric@yahoogroups.com), LV Hayes wrote:

> >From: "Richard Wordingham"

> >Date: Fri, 06 Dec 2002 18:05:50 -0000

> >Subject: [austric] Re: Austric glossary

> >This database records presyllables, but not infixes or  
> >suffixes. Perhaps I'm confounding presyllables and  
> >prefixes, but the database (at  
> ><http://starling.rinet.ru/intrtext.htm>) also seems to  
> >confound them.

> Sergei Starostin's Tower of Babel project at the address you cite

includes

> a large number of etymological databases. I visited the site and

noted

> that since the last time I was there, a Bahnaric database by Ilya

Pejros

> has been added. I assume this is the database to which you refer.

Yes. I'm afraid the identification was scattered across several messages.

> Pejros's Bahnaric citations include diacritics that I do not

understand,

> and I found no legend to them in a brief search of the site. It's a

waste

> of time to try to work with language data written in a

representational

> system that one does not understand.

Diacritic combinations such as 'e|' are defined at  
<http://starling.rinet.ru/code-eng.htm> .

I took final '.T', '.C' and '.L' to be suprasegmental features, though with my limited knowledge I can only guess what they are. To me the obvious guesses are that they are tones or registers; as they don't occur with the Bahnar proto-forms, I didn't think I needed to find out.

> As for your question, the paucity of Munda-AN matches probably

reflects

> foremost the paucity of Munda lexical materials available to search.

Few

> Munda dictionaries exist, and most of them are not huge. As a

consequence,

> many more Munda-AN matches may exist, but remain to be identified by

a more

> thorough search of more copious Munda lexical materials once the

latter are

> available.

Thank you for your explanation.

Richard.